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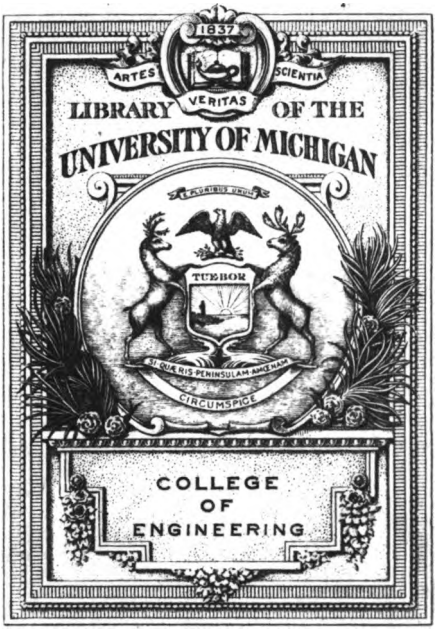

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MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH

ABSTRACTS OF THE DISCUSSIONS.

VOL. XXXII.

SESSION 1870-71.—PART II.

EDITED BY

JAMES FORREST, ASSOC. INST. C.E., SECRETARY.

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THE

INSTITUTION

OF

CIVIL ENGINEERS.

SESSION 1870–71. PART II.

February 14, 1871.

CHARLES B. VIGNOLES, F.R.S., President, in the Chair.

No. 1,207.—"On the Archimedean Screw for lifting Water."[1] By WILFRID AIRY, Assoc. Inst. C.E.

THIS Paper contains the results of a series of experiments undertaken for the purpose of ascertaining the best form of the Archimedean screw, and its effect when laid at different angles of tilt to the horizon. The neglect of the subject hitherto has been doubtless mainly owing to the troublesome nature of the question, for the desired results can be arrived at only by calculation or experiment, and both of these are exceedingly difficult when applied to screws of the ordinary construction. The calculation method would seem to offer insuperable difficulties by reason of the complicated nature of the surfaces involved; this will be readily seen by Figs. 2–5, page 20, which show the successive horizontal sections of the water, as contained in one spiral bend of a screw. The experimental method would involve the construction of a large number of models, and this would be exceedingly troublesome if the threads were set square to the surface of the core, as is usually done.

But on commencing his investigations, it appeared to the Author that there was another principle of forming the spiral threads, which would greatly simplify the work of constructing the

[1] The discussion upon this Paper was taken in conjunction with that upon the following communication on "Centrifugal Pumps," and occupied portions of two evenings.

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necessary models. This was to make the spiral threads on the natural and developable system. If an annular piece of card or tin be wrapped upon a cylindrical core, having its edge retained in a shallow spiral groove on the surface of the core, it will naturally take up a fixed and determinate position; not square to the surface of the core but inclined to it, and inclined to it at an angle depending only upon the inclination of the spiral groove on the core; and it will strongly resist any attempt to make it take any other position. Now the advantages of using this spiral surface are such as to give it an immediate preference over any other. In the first place, it is developable, *i. e.*, it can be made of a single flat piece of plate; so that to construct a spiral thread of this kind no work is required, except to cut out an annulus of plate, which, wrapped upon the core, gives at once the spiral surface: whereas a thread square to the surface of the core is undevelopable; it is of the nature of the surface of a sphere, which cannot be spread out flat, and can only be constructed approximately by using a great number of small pieces. In the next place the developable threads give the best results and produce the most efficient machine: this appeared by experiment, and the results are recorded in Table D, page 15. The two advantages combined appeared to warrant the Author in confining his experiments solely to machines with threads constructed on the developable system. A full discussion of the mathematical properties of the developable spiral surface is given in the Appendix, page 19.

The Archimedean screw consists of three distinct parts, viz., the core, the threads, and the external envelope; and there are four variable quantities, viz., the diameter of the core, the pitch of the spiral on the core, the number of the threads, and the diameter of the external envelope. The pitch of the spiral on the core will be defined by the acute angle which the spiral line makes with lines drawn on the core parallel to its axis, and this angle will be called 'the spiral angle' of the machine. Some of the variable quantities must be fixed by reasons of practical convenience; and in the following investigations the proportion of the internal and the external diameters, and the number of the threads, will be fixed from practical considerations for each machine; while the spiral angle will be left as the variable, which will mainly influence the advantage of the machine, and the effect of which it is the object of the experiments to determine. The external and internal diameters will be the same for all the models, but the number of threads will be generally different for each (for reasons

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to be hereafter described), and each machine will have its own spiral angle.

The models for the experiments were carefully constructed of wood and tin. They were each placed in succession on a horizontal board, and tilted in a frame to successive angles of inclination to the horizon. The volume of water which was held by each model in its successive positions was accurately measured, by means of a graduated measuring glass.

In the first series of experiments the machines were constructed in their simplest form, viz., with one thread only apiece. Four models were made, and the results obtained are shown in Table A, page 9. The chief facts deduced from this series of experiments are as follow:

1st. That the smaller the spiral angle, *i. e.*, the quicker the spiral, the flatter must the machine be laid to the horizon to produce its best effect.

2nd. That in an equal number of revolutions, the quicker spirals will lift much more water than the slower ones.

3rd. That there is a great difference in the effect of the machines according as one end or the other is upwards. The advantage is greatly in favour of the machine when placed so that the acute angle which the thread makes with the core is downwards. In all the experiments that are to follow, the machines are to be supposed placed with the acute angle downwards.

The foregoing experiments have been made for machines having one thread only, but a little consideration will show, that every machine should have as many threads as the conditions of ordinary workmanship and convenience will allow. This is seen by the results given in Table C, page 14, where the advantage is shown of increasing the number of threads from 3 to 4. Now, in order to allow of easy fitting, riveting, and examination, as well as to avoid the addition to the weight and complexity of the machine which would be produced by a great number of threads, the Author has concluded that, for a screw of large size, say 6 feet or 7 feet external diameter, the width of the chambers should not be less than 18 inches on the square; and the diameter of the core has been fixed at one-third nearly of the external diameter. These conditions will limit the number of threads as follow:—

For a screw whose spiral angle is . . .	20°	4 threads.
" " " . . .	30°	4 "
" " " . . .	40°	3 "
" " " . . .	50°	3 "
" " " . . .	60°	2 "
" " " . . .	74°	1 thread.

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In the second series of experiments, the models were constructed each with its full natural advantages as regards number of threads. Six models were made. The external and internal diameters were the same for each, and the spiral angles were respectively 20°, 30°, 40°, 50°, 60°, and 74°. (The reason for selecting 74° rather than 70° in the last model was to avoid a great irregularity which must have existed in the width of the chambers if 70° had been used.)

The machines being each tilted and filled gave the results recorded in Table B, page 11, which suggest the following remarks:—

1st. Comparing the results of Table B with those of Table A, the effect of using several threads instead of one has been to improve each machine, so that it can be advantageously applied at a steeper angle of tilt than before.

2nd. Screws of quick spiral angle, when laid at their best angle of tilt, carry a far greater volume of water than those of slower spiral angle when laid at their best angle of tilt. If, therefore, the object is to lift the greatest quantity of water in a given time, a machine of quick spiral angle must be provided and laid at a flat angle of tilt.

3rd. If the size of the machine, *i. e.*, the external diameter, be a fixed quantity, then Table B will show at once the best form of machine for lifting a given quantity of water to a given height. This will clearly be the machine that lifts the quantity required when laid at the steepest angle, because such a machine would be shorter and lighter than any other.

It now remains to investigate the losses due to the internal and external friction in the several machines, in order to determine which form of screw is the most economical. In the following comparisons, it is assumed that each screw is laid at its best angle of tilt to the horizon, as already determined, and that all are turned at the same rate.

The effect of the internal friction due to the water passing up the screw may be arrived at as follows: Suppose that the power which drives the machine is commuted for a head of water which would produce the actual discharge, by forcing water through the spiral chambers of the screw in a continuous stream. The case may then be considered as that of a fixed screw leading from the bottom of a reservoir in which the water stands so high as to force water with the correct velocity through the spiral canal of the machine. In illustration of the artifice here adopted: Let A B (Fig. 1) be the

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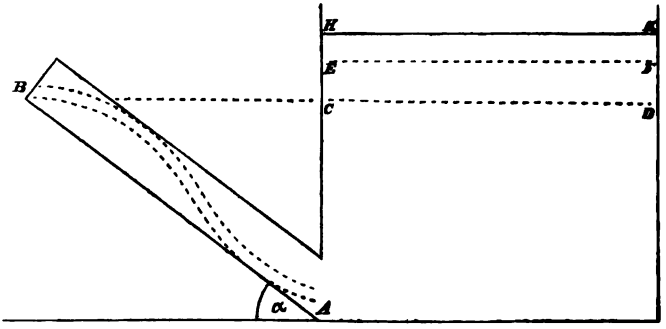
Fig. 1.



machine inclined at its best angle of tilt (α). The dotted lines A B represent the course of a spiral canal up the machine, and A K is the reservoir with which the screw is supposed to be connected. The column of water which would be required to force water up to B would be A C; the addition to this column required to force the water through the machine with the correct velocity is C E; and the further addition to the column required to overcome the frictional resistance of the water in the canal is E H. The value of the frictional resistance represented by E H has been determined for each machine, in accordance with the theory and constants obtained by Du Buat. The results are given in Table E, page 15. Table F, page 17, shows the heads necessary for producing the required velocity through the screws. In order to form these Tables it was necessary to assume certain data for the comparison, and the following were selected :—

Height of lift	10 feet.
Diameter of external envelope	3 „
Diameter of core	1 foot.
Rate of revolution	20 turns per minute.

With these data it appears that the effect of the internal frictional resistance is to increase the work, by a quantity varying from $4\frac{1}{2}$ to 8 per cent. of the useful effect realized.

The external frictional resistance on the gudgeons has been calculated for each machine, and the results appear in Table G, page 17. The numerical data have been assumed as before, and in addition—

Diameter of gudgeons	4 inches.
Coefficient of friction	·12
Thickness of plates	$\frac{3}{16}$ inch.

The results are given in foot-pounds of friction, and it appears

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that the effect of the external friction is to increase the work, by a quantity varying from 12 per cent. upwards of the useful effect realized.

The efficiency of each machine will be measured by the proportion in which its effect is reduced by friction, *i. e.*, by the proportion which the power required to drive the machine bears, to the power which would have been required if no friction had acted. Combining the results obtained for internal and external friction, the efficiencies of the machines have been calculated, and the results are given in Table H, page 18. From these it appears that the machine whose spiral angle is 30° is the most economical, but the machine of which the spiral angle is 40° approaches it very closely; and, indeed, when a slight alteration of the conditions is made (by taking 3-inch gudgeons instead of 4-inch), the results are absolutely the same for both machines. It may therefore be taken that, for the most economical machine, the spiral angle should not be less than 30° nor greater than 40°, and that the economy of the machine will be affected but very slightly by using intermediate spiral angles.

By reference to Table H, it appears that, in the most favourable case, the useful effect of the Archimedean screw is 88 per cent. Taking account of small losses, such as the resistance of the water in which the screw dips, which will be the same for each machine, the useful effect of a well-constructed screw placed at the best angle ought not to be less than 85 per cent. Such a result would contrast most favourably with those given by any machine in common use for low lifts, and, for the purpose of comparison, it may not be superfluous to refer to these in order. The machines most commonly used for low lifts, are as follow: suction pumps, centrifugal pumps, Archimedean screws, scoop wheels, chain pumps, and Persian wheels.

Suction Pumps.—The best of these give 80 per cent. of useful effect. They need, however, constant attention to keep them in order: the packing of the piston requires continual renewal, and the valves wear out and get damaged by pieces of wood and other floating substances. This has been found so great an objection that, for fen-drainage, both in England and Holland, machines of simpler character and needing less repair have been adopted, although at a considerable loss in point of economical working.

Centrifugal Pumps.—These admirable pumps give excellent results, and for the rapid and economical performance of a continuous work there are none better. They need, however, to be driven at a high rate of speed, and have not as yet displaced the older machines for

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fen-drainage. It seems likely that, for work of an irregular character, the less perfect machines, such as the scoop wheels and the Archimedean screws as employed by the Dutch, may still hold their ground.

Archimedean Screws.—These are the machines most commonly used by the Dutch for permanent drainage. The screws are made without the external envelope, and play close to a semi-cylinder of masonry, which supplies the place of the external envelope. This is not an economical machine: the leakage between the screw and the masonry is a serious drawback, and the useful effect will rarely exceed 60 per cent. The machine has also to be driven at a considerable speed, and in all respects it is far inferior to the Archimedean screw as closed in by an external envelope.

Scoop Wheels.—These are the machines most generally adopted for fen-drainage in England. They are very convenient, and are admirably adapted for lifting large quantities of water to a limited height. They are however wasteful of power: the leakage between the wheel and the walls is a serious loss, and the wheels frequently throw the water much higher than is necessary. It is not probable that any one of the wheels now at work in the fens realizes more than 60 per cent. of useful effect.

Chain Pumps and Norias.—These machines are not commonly used for permanent work, nor have they been constructed of large size. The number of joints and wearing parts forms an objection to their use. The noria in its best form has given a useful effect of 75 per cent.

Persian Wheels and Tympans.—The former of these consists of a wheel set round with buckets, and is frequently used for irrigation purposes, but there are no certain data as regards the useful effect: there is no doubt but that it can be made highly economical. The tympan, which takes in water at its circumference and delivers it through its hollow axle, is adapted only for very low lifts and moderate quantities of water. Its useful effect has been found to be 84 per cent. in one instance; it appears, however, to be but little used.

The objects of the Author, in offering to the Institution the results of his experiments, were to introduce the superior advantages of the Archimedean screw, when constructed with developable threads, and to exhibit its effect when so constructed. There can be no doubt but that the labour of constructing screws with threads square to the surface of the core would be so great as to form a serious objection to their general use; but by employing developable threads the construction is greatly simplified, and the

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advantages of thin iron plate as the material are at once perceived. This appears of great importance; for if the machines are to be introduced, they must be durable and well constructed, and this can only be done in iron. The machines appear well worthy of greater notice than they have received in England. For low lifts they are probably simpler and more effective than any other sort of machine. The fewness of parts and their ready erection with little skilled labour render them peculiarly suitable for drainage and irrigation in distant countries, while their compactness and lightness would seem to fit them well for exportation and transmission to inland parts. They are little liable to choakage or accidents of any kind, and if durably constructed would probably continue to work for many years without need of repairs and with little loss of effect. They are readily driven by any sort of power, and from their effective working at slow rates of speed are very suitable for hand-labour, which in many countries is the cheapest form of power and the most readily applied.

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APPENDIX.

TABLE A.

EXPERIMENTS on MACHINES having ONE THREAD only.

In this Table, and in subsequent Tables, the numbers expressing the volumes represent fluid ounces of water.

By "obtuse angle downwards," or "acute angle downwards," is meant that the machine is so placed that the obtuse or acute angle which the thread makes with the core is downwards.

The asterisk against the last column distinguishes the best result in each case.

In each case the external diameter was 6 inches, and the diameter of the core $1\frac{3}{4}$ inch.

No. 1.—Spiral Angle of Machine, 40°.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Bend. Obtuse Angle downwards.	Volume contained in one Spiral Bend. Acute Angle downwards.	Height of Lift for length of Machine unity.	Product of Numbers in last two Columns.
°				
15	..	36·00	·259	9·32
20	17·25	27·70	·342	9·47*
25	..	20·87	·422	8·81
30	4·00	15·50	·500	7·75
40	0·20	8·62	·643	5·54
50	0·00	2·70	·766	2·07
60	0·00	0·37	·866	0·32

No. 2.—Spiral Angle of Machine, 50°.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Bend. Obtuse Angle downwards.	Volume contained in one Spiral Bend. Acute Angle downwards.	Height of Lift for length of Machine unity.	Product of Numbers in last two Columns.
°				
20	21·55	26·50	·342	9·06
25	..	23·25	·422	9·81*
30	9·30	18·00	·500	9·00
40	2·90	10·40	·643	6·69
50	0·37	5·70	·766	4·37
60	0·00	1·40	·866	1·21

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No. 3.—Spiral Angle of Machine, 60°.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Bend. Obtuse Angle downwards.	Volume contained in one Spiral Bend. Acute Angle downwards.	Height of Lift for length of Machine unity.	Product of Numbers in last two Columns.
°				
20	19·15	21·00	·342	6·91
25	..	19·25	·423	8·14*
30	12·87	16·10	·500	8·05
35	..	13·75	·573	7·88
40	6·62	11·50	·643	7·39
50	2·70	7·40	·766	5·67
60	0·35	3·75	·866	3·25

No. 4.—Spiral Angle of Machine, 70°.

Angle of Tilt to horizontal.	Volume contained in one Spiral Bend. Obtuse Angle downwards.	Volume contained in one Spiral Bend. Acute Angle downwards.	Height of Lift for Length of Machine unity.	Product of Numbers in last two Columns.
°				
20	12·75	14·75	·342	5·13
30	11·25	13·12	·500	6·56
35	..	12·50	·574	7·17
40	8·75	11·70	·643	7·52*
45	..	10·20	·707	7·21
50	4·75	9·25	·766	7·08
60	1·95	6 35	·866	5·50
70	0·00	2·80	·940	2·63

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TABLE B.

EXPERIMENTS on MACHINES having several THREADS, the number and size of CHAMBERS being suited to the conditions of each MACHINE, as described in the text.

In this set of Experiments the Machines were in every case placed with the acute angle of the thread downwards.

The asterisk against the last column distinguishes the best result in each case.

In each case the external diameter was 7 inches, and the diameter of the core 2·2 inches.

No. 1.—Spiral Angle of Machine, 20°.
Number of Chambers . 4.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Chamber.	Volume delivered per Revolution of Machine.	Height lifted for length of Machine unity.	Product of Numbers in last two Columns.
°				
10	46·50	186·00	·173	32·29
15	37·25	149·00	·259	38·59*
20	24·75	99·00	·342	33·86
25	11·70	46·80	·423	19·80
30	4·40	17·60	·500	8·80
35	1·25	5·00	..	..
40	0·20	0·80	..	..
45	0·00	0·00	..	..

No. 2.—Spiral Angle of Machine, 30°.
Number of Chambers . 4.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Chamber.	Volume delivered per Revolution of Machine.	Height lifted for length of Machine unity.	Product of Numbers in last two Columns.
°				
15	30·00	120·00	·259	31·08
20	26·90	107·60	·342	36·80
25	22·90	91·60	·423	38·75*
30	17·75	71·00	·500	35·50
35	10·60	42·40	·574	24·33
40	5·40	21·60	..	..
45	2·05	8·20	..	..
50	0·45	1·80	..	..
55	0·20	0·80	..	..
60	0·00	0·00	..	..

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No. 3.—Spiral Angle of Machine, 40°.
Number of Chambers . 3.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Chamber.	Volume delivered per Revolution of Machine.	Height lifted for length of Machine unity.	Product of Numbers in last two Columns.
°				
15	28·50	85·50	..	..
20	26·60	79·80	·342	27·29
25	23·50	70·50	·423	29·82
30	20·60	61·80	·500	30·90*
35	17·65	52·95	·574	30·39
40	14·75	44·25	·643	28·45
45	9·00	27·00	..	..
50	4·40	13·20	..	..
55	1·85	5·55	..	..
60	0·35	1·05	..	..
65	0·10	0·30	..	..

No. 4.—Spiral Angle of Machine, 50°.
Number of Chambers . 3.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Chamber.	Volume delivered per Revolution of Machine.	Height lifted for length of Machine unity.	Product of Numbers in last two Columns.
°				
15	22·58	67·75	..	..
20	20·75	62·25	·342	21·28
25	19·25	57·75	·423	24·43
30	17·58	52·75	·500	26·37
35	15·66	47·00	·574	27·00*
40	13·60	40·80	·643	26·23
45	11·80	35·40	·707	25·03
50	9·83	29·50	·766	22·60
55	6·40	19·20	..	..
60	2·90	8·70	..	..
65	0·87	2·60	..	..
70	0·16	0·50	..	..
75	0·00	0·00	..	..

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No. 5.—Spiral Angle of Machine, 60°.
Number of Chambers . 2.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Chamber.	Volume delivered per Revolution of Machine.	Height lifted for length of Machine unity.	Product of Numbers in last two Columns.
°				
15	23·20	46·40	..	..
20	21·85	43 70	·342	14·95
25	20·75	41·50	·423	17·55
30	19·50	39·00	·500	19·50
35	17·80	35·60	·574	20·43*
40	15·87	31·75	·643	20·41
45	14·00	28·00	·707	19·79
50	11·50	23·00	·766	17·62
55	9·57	19·15	..	..
60	7·50	15·00	..	..
65	4·12	8·25	..	..
70	1·65	3·30	..	..
75	0·27	0·55	..	..
80	0·00	0·00	..	..

No. 6.—Spiral Angle of Machine, 74°.
Number of Chambers . 1.

Angle of Tilt to Horizontal.	Volume contained in one Spiral Chamber.	Volume delivered per Revolution of Machine.	Height lifted for length of Machine unity.	Product of Numbers in last two Columns.
°				
15	..	23·50	..	..
20	..	22·60	..	..
25	..	21·70	..	..
30	..	21·00	·500	10·50
35	..	20·05	·574	11·51
40	..	19·15	·643	12·31
45	..	17·80	·707	12·58*
50	..	16·10	·766	12·33
55	..	14·52	·819	11·89
60	..	12·60	·866	10·91
65	..	10·20	..	..
70	..	6·75	..	..
75	..	2·65	..	..
80	..	0·50	..	..

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TABLE C.

EXPERIMENT to determine the effect of INCREASING the number of THREADS from THREE to FOUR in the case of the Machine whose SPIRAL ANGLE is 40°.

Angle of Tilt to Horizontal.	Volume delivered per Revolution by Machine with Four Threads.	Volume delivered per Revolution by Machine with Three Threads.	Advantage of Machine having Four Threads.
°			
15	86·75	85·50	1·25
20	81·20	79·80	1·40
25	74·10	70·50	3·60
30	68·40	61·80	6·60
35	60·00	52·95	7·05
40	53·25	44·25	9·00
45	37·50	27·00	10·50
50	21·75	13·20	8·55
55	10·65	5·55	5·10
60	3·00	1·05	1·95
65	0·50	0·30	0·20
70	0·00	0·00	0·00

REMARKS.

From this Table it appears that for the same angle of tilt (in the neighbourhood of the angle of best effect) the machine having four spiral chambers will carry about 10 per cent. more water than the machine with three chambers only.

Also the effect of using four threads instead of three is seen in improving the angle of best effect from 30° to 35°, as may be found on trial.

And since the volume carried by the machine having four threads, when laid at an angle of 35°, is nearly as great as that carried by the machine having three threads, when laid at an angle of 30°, the improvement might be estimated as a net angular improvement of nearly 5° of elevation.

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TABLE D.

EXPERIMENT to compare the effect of a MACHINE constructed with the SPIRAL THREADS square to the surface of the Core, with that of a MACHINE (*see* Table B, No. 3) whose THREADS are constructed on the DEVELOPABLE SYSTEM, all other quantities being alike for both Machines.

Spiral angle of each Machine, 40°.
Number of chambers for each, 3.

Angle of Tilt to Horizontal.	Volume delivered per Revolution by Machine having developable Threads.	Volume delivered per Revolution by Machine having Threads square to surface of Core	Advantage of Machine having developable Threads.
°			
15	85·50	73·95	11·55
20	79·80	66·15	13·65
25	70·50	54·60	15·90
30	61·80	41·70	20·10
35	52·95	28·80	24·15
40	44·25	16·50	27·75
45	27·00	11·25	15·75
50	13 20	5·70	7·50
55	5·55	3·60	1·95
60	1·05	0·95	0·10
65	0·30	0·30	0·00

REMARKS ON TABLE D.

Hence it appears that at the best angle of tilt (30°) the machine with developable threads will lift half as much again as the machine having threads square to the surface of the core.

TABLE E.

ELEMENTS for computation of INTERNAL FRICTIONAL RESISTANCE of WATER, and RESULTS obtained for each MACHINE when laid at its best angle of effect.

In the following Table the data for the calculation are as follow:—

Diameter of external envelope . . 3 feet.
Diameter of core . . . . . 1 foot.
Rate of revolution . . . . . 20 turns per minute.

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In explanation of the various terms which compose the Table, it must be observed that—

1. The volume of water contained in a spiral bend is assumed to bear the same proportion to the whole volume of the bend as was the case with the experimental models.

2. The "mean length of spiral column per turn" is in each case the length of the spiral on the external envelope, minus $\frac{1}{4}$th of the difference between the lengths of the spirals on the external envelope, and on the core per turn respectively.

3. The wetted perimeter was taken graphically from diagrams showing the section on square of the chambers of the experimental models.

4. The frictional resistance was calculated from the formula—

$$\text{Frictional resistance} = \cdot 0001 \cdot \frac{c \cdot l}{s} (v^2 + \cdot 20\, v),$$

where

c = wetted perimeter of canal,
l = whole length of canal,
s = sectional area of stream,
v = velocity of water in feet per second,

and appears in the shape of a linear addition to the lift.

Nos. of Machines as in Table B	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Spiral angle of Machines	20°	30°	40°	50°	60°	74°
Angle of tilt to horizontal	15°	25°	30°	35°	35°	45°
Volume delivered per chamber per revolution	5·35	3·24	2·94	2·22	2·53	2·53
Mean length of spiral column per turn	12·08	9·96	9·06	8·65	8·40	8·22
Sectional area of column of fluid	·443	·325	·324	·257	·301	·308
Wetted perimeter of spiral chamber	2·55	2·07	2·02	1·59	1·66	1·62
Length of spiral column per foot run of machine	1·395	1·830	2·423	3·280	4 640	9·133
Velocity of water in feet per second	4·026	3·320	3·020	2·883	2·800	2·740
Frictional resistance per foot run of machine	·0137	·0136	·0147	·0180	·0215	·0388
Frictional resistance per foot of lift	·0527	·0322	·0294	·0314	·0375	·0549

In this Table all the numbers are expressed in feet, and the final result is the linear addition per foot of lift to be made to the net height lifted in order to overcome the frictional resistance of the water passing up the machines.

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TABLE F.

This Table shows the whole addition that must be made to the net height lifted, in order that the water may be discharged with the required velocity, and may overcome the internal frictional resistance.

Nos. of Machines as in Table B	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Spiral angle of Machines	20°	30°	40°	50°	60°	74°
Angle of tilt to horizontal	15°	25°	30°	35°	35°	45°
Velocity of water in feet per second	4·026	3·320	3·020	2·883	2·800	2·740
Head due to velocity, in feet	·2518	·1712	·1416	·1290	·1217	·1170
Head due to internal friction (Table E)	·0527	·0322	·0294	·0314	·0375	·0549
Sum of both heads for height of lift 10 feet	·779	·493	·436	·443	·497	·666

TABLE G.

ELEMENTS for computation of EXTERNAL FRICTION on the GUDGEONS of the MACHINES, and effect of external friction calculated for each Machine when laid at its best angle of effect.

The data for the calculation are as follow :—

Diameter of external envelope	3 feet.
Diameter of core	1 foot.
Radius of gudgeons	2 inches.
Coefficient of friction	·12
Thickness of plates everywhere	$\frac{1}{16}$ inch.

And the Table was calculated by the formula :

$$\left.\begin{array}{l}\text{Mechanical effect of friction}\\ \text{per revolution}\end{array}\right\} = \mu\,(w + w')\cdot h\cdot \cot a \times 2\pi r,$$

where

a = angle of tilt of machine,
w = weight of machine per foot run,
w' = weight of water in machine per foot run,
r = radius of gudgeons,
μ = coefficient of friction,
h = height of lift.

The numbers throughout the Table represent lbs. avoirdupois, and the final result is the resisting force of friction multiplied by the circumference of the gudgeons, this being the distance through which it acts per turn of the machine.

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Nos. of Machines as in Table B	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Spiral angle of Machines	20°	30°	40°	50°	60°	74°
Weight of water contained in a spiral bend	1,337	810	551	416	316	158
Weight of water contained per foot run of Machine	154	149	147	158	174	175
Weight of Machine per foot run	220	220	190	190	160	130
Sum of last two quantities	374	369	337	348	334	305
Force of friction per foot of lift	167	95	70	60	57	37
Mechanical effect of friction per foot of lift—per revolution	175	99	73	63	60	39

TABLE H.

This is a Table of the values of the expression

$$\frac{P'}{P} = \frac{W(h + mh + k) + nh}{W(h + k)}$$

for the efficiency of the machines, and is calculated for each of the machines when laid at its best angle of effect, the height of lift being 10 feet in each case.

In this formula:

P' = power required to lift a weight of water (W) to a height (h) at each revolution.

P = power which would have been required if no friction, external or internal, had acted.

mh = increment of lift due to internal friction of water.

k = increment of lift due to velocity of water through the machine.

nh = external frictional resistance.

Nos. of Machines as in Table B	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6
Spiral angle of machines	20°	30°	40°	50°	60°	74°
Angle of tilt to horizontal	15°	25°	30°	35°	35°	45°
Values of expression $\frac{P'}{P}$ with 4″ gudgeons	1·178	1·156	1·159	1·180	1 225	1·300
Values of expression $\frac{P'}{P}$ with 3″ gudgeons	1·147	1·126	1·126	1·142	1·178	1·235

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Sketches of Successive Horizontal Sections of the Water, as contained in a Spiral Chamber of a Machine.

Four of these are given. Fig. 2 is a plan of the surface when the chamber is full, and the other sections were obtained by drawing off some of the water. The outside dotted lines show the external diameter of the machine, and the inner dotted lines show the core. The model which furnished the sections was that whose spiral angle is 40° (Table B, No. 3), and it was tilted to an angle of 30° to the horizontal.

[For Figs. 2, 3, 4, 5, see page 20.]

Mathematical Properties of Spiral Surfaces.

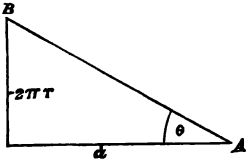
1. A uniform spiral line upon a cylinder is a line traced out by a point whose progression in the direction of the cylinder's length bears a constant proportion to its progression in the direction square to the cylinder's length, *i.e.* along the circumference of the cylinder.

2. This is also the property of a straight line with reference to co-ordinate axes: hence, if upon a piece of paper there be drawn a straight line making with another line a given angle, and this paper be wrapped upon a cylinder so that the second line lies upon the cylinder parallel to its axis, the first line will show upon the cylinder as a uniform spiral line cutting all the generating lines of the cylinder successively at the angle aforesaid.

3. This angle—the acute angle which the spiral line makes with the generating lines of the cylinder—is throughout the foregoing Paper called "the spiral angle;" the relation between the spiral angle and the linear dimensions is as follows:—

Let θ = the spiral angle;
r = Rad. of cylinder;
a = Progression per complete turn in direction of cylinder's length.

Fig. 6.



Then, since the tracing point of the spiral has progressed through a distance (a) in the direction of the cylinder's length while it has progressed through a distance ($2\pi r$) in the direction square to that length, the spiral traced out is that which if unwrapped from the cylinder would show as a straight line AB inclined to the first of these directions at an angle (θ) such that $\tan\theta = \frac{2\pi r}{a}$.

4. Since the spiral line cuts the generating lines of the cylinder always at the same angle, the curvature of the spiral is everywhere the same. Now if (ρ) be the radius of curvature of a skew section of a cylindrical surface inclined at an angle (ϕ) to the section taken at right angles to the axis, the relation existing between the radii of curvature is

$$\rho = \frac{r}{\cos^2\phi}.$$

Hence, taking (ρ) to represent the constant radius of curvature of a spiral line which cuts the generating lines of the cylinder at an angle θ, and observing that $\theta = 90° - \phi$, we have

$$\rho = \frac{r}{\sin^2\theta}.$$

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Fig. 2.

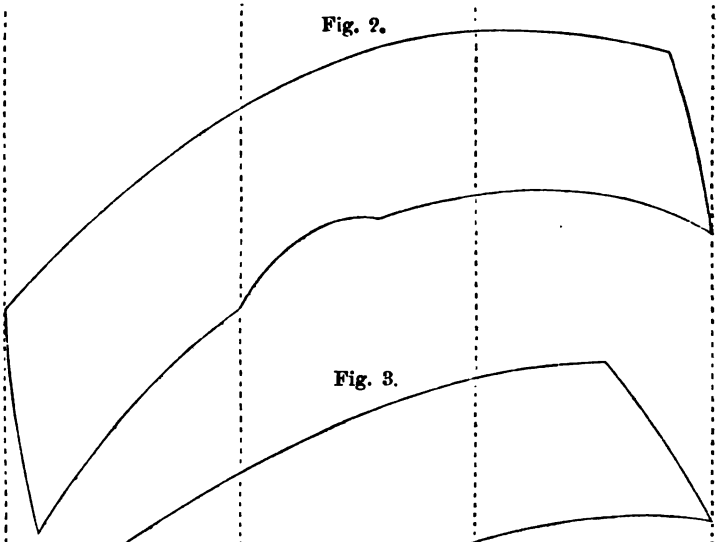

Fig. 3.

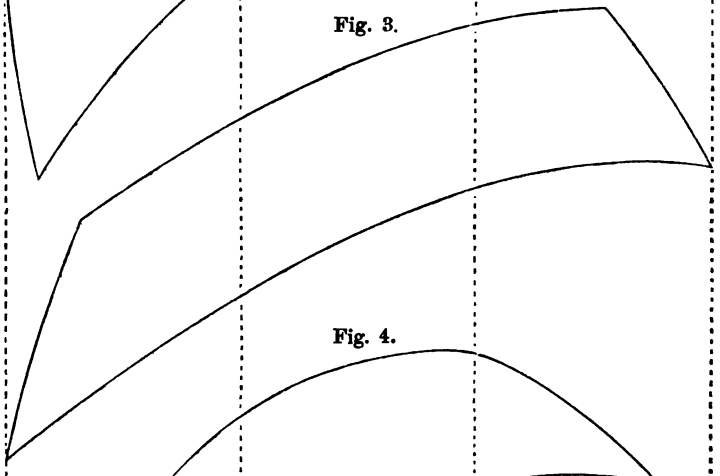

Fig. 4.

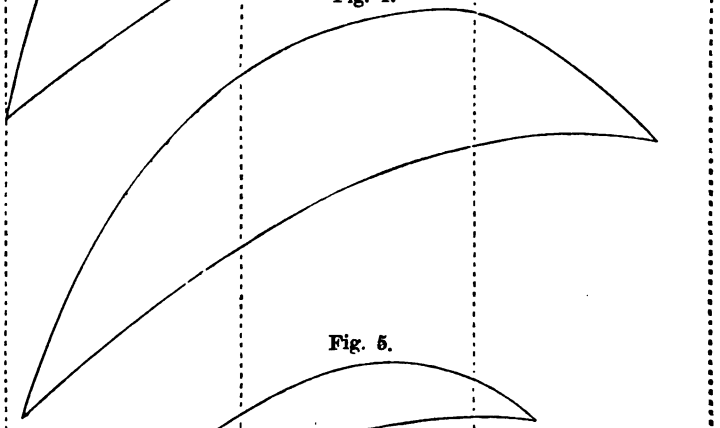

Fig. 5.

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5. If therefore an annulus of card be cut out, the inner circle of which is struck to the radius of curvature found as above, and if the inner edge of the card be applied to the cylinder and be guided into the spiral line by a slight groove, it will naturally wrap close around the cylinder, and the annular card will become a spiral surface surrounding the cylinder. This spiral surface, however, will not be square to the surface of the cylinder, but inclined to it, and the degree of its inclination will depend on the spiral angle; this may be seen as follows:

6. Since a uniform spiral line has everywhere the same radius of curvature, the point which traces out the spiral line may be considered as continually tracing out a circle whose radius is the radius of curvature, $\frac{r}{\sin^2\theta}$, as found above (4). If therefore an annulus of card be applied to the spiral line, it is, as it were, applied along the circumference of a circle of that radius. And if an annulus of card be supposed applied to the circumference of a disc, the only position which such an annulus would be able to maintain would be in the plane of the disc, because such a position is the only one that would suit the curvature of the successive concentric curves of the card. Therefore the card annulus, when applied to the spiral line on the cylinder, will endeavour to follow the same law, *i.e.* it will endeavour to keep itself always in the plane containing the circle of curvature of the spiral line: and its natural and only position is in the successive planes which contain the spiral line, or, as they are called, in the successive osculating planes of the curve. Now these successive osculating planes by their continual intersections generate a spiral surface, and it is this spiral surface which the annulus of card will assume when wrapped upon the cylinder. Also since the osculating plane always contains the tangent to the curve, the spiral surface may be considered as generated and traced out by the tangent to the spiral curve on the cylinder. Hence we arrive at a simple method of describing the spiral surface which an annulus of card will assume when wrapped along a uniform spiral line on a cylinder. Draw upon a stiff card or thin board a straight line making the spiral angle with another line also drawn upon it: apply the board to the cylinder so that the second line is parallel to the axis of the cylinder: roll the board steadily round the cylinder, and as the straight line traces out the spiral line on the surface of the cylinder, its prolongation (which is of necessity a tangent to the curve) will trace out the spiral surface which would be assumed by an annulus of card wrapped upon the cylinder along the spiral line.

7. It will easily be seen from the foregoing reasoning that the spiral surface which the annulus assumes is the *only* spiral surface which can be formed out of a single piece of card, *i.e.*, it is the only possible developable spiral surface which can be applied to a given spiral line: all other surfaces are undevelopable, and must be constructed approximately by using a great number of small pieces: as regards the spiral surface, which is everywhere square to the surface of the core, this may be easily shown as follows:

Since the card is an annulus, and its inner edge is struck to a radius (ρ), let $b =$ its breadth. Then the radius of its outer edge is $(\rho + b)$, and from sections (3) and (4)

$$\rho + b = \frac{r}{\sin^2\theta} + b, \text{ and } \tan\theta = \frac{2\pi r}{a};$$

$$\therefore \text{ radius of outer edge of card} = r \cdot \frac{a^2 + 4\pi^2 r^2}{4\pi^2 r^2} + b.$$

Now if it can be shown that the radius of curvature of a spiral line on a cylinder of radius $(r + b)$ is different from the radius of the outer edge of the card, as above

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found, it will follow that the outer edge of the card cannot occupy the position of the spiral line, *i. e.*, it cannot stand square to the surface of the core.

Now the radius of curvature of a uniform spiral traced on a cylinder of radius $(r + b)$ is (see sect. 4) $\rho' = \frac{r + b}{\sin^2 \theta'}$ (where θ' = the spiral angle); and since the progression of the outer edge of a spiral surface in the direction of the cylinder's length must be the same as that of the inner edge which is wrapped upon the core, we shall have $a' = a$ and $\tan \theta' = \frac{2\pi (r + b)}{a}$, and therefore

$$\rho' = (r + b) \cdot \frac{a^2 + 4\pi^2 (r + b)^2}{4\pi^2 (r + b)^2}.$$

And this radius of curvature will be greater or less than the radius of curvature of the outer edge of the card annulus, according as

$$(r + b) \cdot \frac{a^2 + 4\pi^2 (r + b)^2}{4\pi^2 (r + b)^2} \gtrless r \cdot \frac{a^2 + 4\pi^2 r^2}{4\pi^2 r^2} + b;$$

$$\text{or, } (r + b) \cdot \frac{a^2}{4\pi^2 (r + b)^2} \gtrless r \cdot \frac{a^2}{4\pi^2 r^2}$$

$$\text{or, } \frac{1}{r + b} \gtrless \frac{1}{r}.$$

Hence it is seen the curvature of the spiral line at a distance (b) from the surface of the core is greater than the curvature of the outer edge of the card: consequently the edge of the card cannot occupy that position, and the card cannot, therefore, stand square to the surface of the core: such a spiral surface must, therefore, be undevelopable.

The same proposition may be proved by showing that the lengths of spirals on the cylinders whose radii are r, $r + \frac{b}{2}$, $r + b$, do not increase in arithmetical progression, as is the case with the concentric curves of the card annulus: therefore the card when wrapped upon the core must necessarily buckle.

PROBLEM I.

Required the radius of curvature for the outer edge of an annulus of card so that, when wrapped upon a given spiral line on a given cylinder, it shall project from the surface of the cylinder to a given distance, so as to give rise to a machine of definite external diameter.

This problem will be readily solved by means of the simple property of developable spiral surfaces mentioned in sect. 6 :

Let r = radius of cylinder.
a = the given spiral angle.
R = the required external radius for the machine.

Let AB be the cylinder in plan, and let CD be a board as described in sect. 6 touching the cylinder along the line EF. Let GH be the line drawn upon the board which traces out the spiral surface, and let P be the point where the line touches the cylinder, and Q a point on the outer edge of the card annulus.

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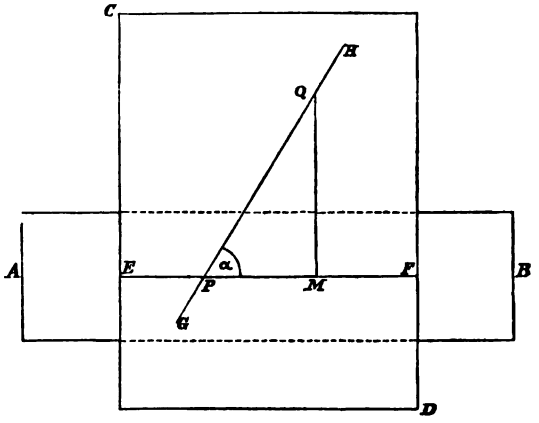
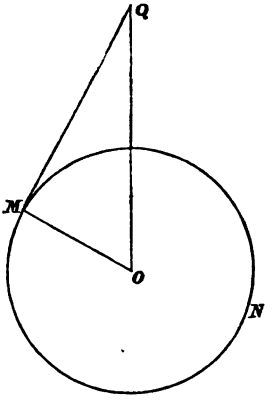
Let QP = x, and from Q let fall in the plane of the board the perpendicular QM upon EF, meeting EF in M. Then QM = $x \sin a$.

Fig. 7.



Now QM is a tangent to a section taken at right angles to the cylinder AB at M. Let, therefore, MN (Fig. 8) be a section on square of the cylinder AB at M,

Fig. 8.



and QM the tangent, as in Fig. 7. Let O be the centre of the circle MN. Then OM = r, and OQ = R, the given external radius; and

$$O M^2 + Q M^2 = O Q^2$$

$$r^2 + x^2 \sin^2 a = R^2$$

$$\therefore \qquad x = \frac{\sqrt{R^2 - r^2}}{\sin a}.$$

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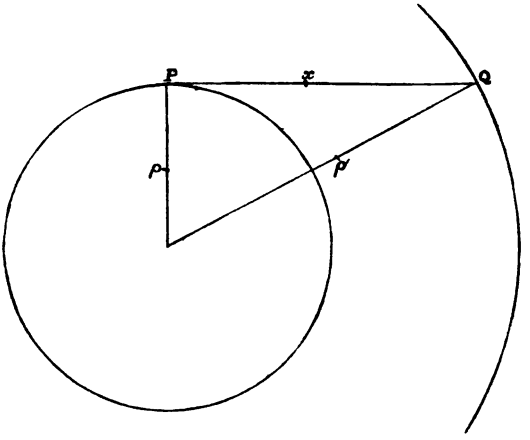
Now observing that QP or x is the length of a tangent to the spiral line on the cylinder drawn to a point Q, on the outer edge of the spiral surface—

Let ρ = radius of curvature of the spiral line on the cylinder.
ρ' = radius of curvature of the outer edge of the annulus.

Then from Fig. 9 we shall have evidently—

Fig. 9.



$$\rho'^2 = \rho^2 + x^2$$

or,
$$\rho' = \sqrt{\rho^2 + \frac{R^2 - r^2}{\sin^2 a}},$$

the radius required.

PROBLEM II.

To find how many threads can be laid upon a given core so as to preserve a given space between adjacent threads.

Let AB, CD, be two adjacent spirals as unwrapped from the core, AF being a line which would be parallel to the axis of the core and AC perpendicular to AF. Draw CE perpendicular to AB. Then CE will represent the space between adjacent threads, and AC will be the length of circumference of the core occupied between the two spiral lines.

Let r = rad. of core.
θ = spiral angle of threads.
w = width of chambers on square.

Then since AC = $w \cdot \sec \theta$,
And circumf. of core = $2 \pi r$,
Therefore number of threads that can be laid on the core is

$$\frac{2 \pi r}{w \cdot \sec \theta}.$$

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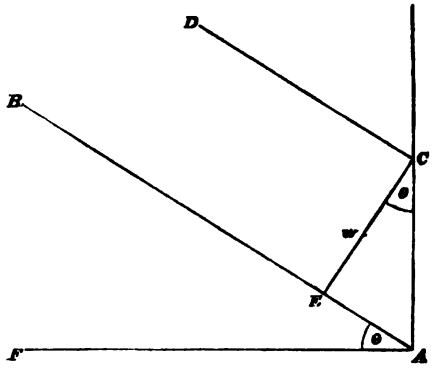
Fig. 10.



Corollary.—If the number of threads be given, and it be required to determine the width of chambers that will result—

Let n = the number of threads.

Then $\frac{2\pi r}{n}$ = length of circumference due to a chamber = A C.

And width of chambers $= A C \cdot \cos \theta.$

$$= \frac{2\pi r}{n} \cdot \cos \theta.$$

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No. 1,290.—"Centrifugal Pumps."[1] By DAVID THOMSON, Assoc. Inst. C.E.

No Paper has hitherto been presented to the Institution on this subject, and very little has been published, with reference to it, which is either generally accessible or of ready application in practice. The Author has accordingly embodied in this communication the information he has acquired from various sources, tested and extended by the experience gained in the course of his own practice.

The centrifugal pump appears to have been invented about one hundred and twenty years ago. It was used in America in 1830;[2] and one was erected in the American Navy Yard, New York, in 1838. The curved form of vanes was at first adopted in some of the American pumps, but it was afterwards discarded for the straight form. About that time M. Ch. Combes, Member of the Institute of France, discussed the relations between the centrifugal ventilator and the centrifugal pump, and insisted on the curved form proper to the vanes, which subsequent experiments with models of the centrifugal pump, made between 1838 and 1843, confirmed.

It may be added that an ingenious Scotch Engineer, Mr. James Whitelaw, the inventor and patentee of an improved form of Barker's mill, conceived the idea, about the year 1846, of reversing the Barker's mill, and driving it so as to form a centrifugal pump. He made various experiments on its performance, by means of a working model, detailed accounts of which were published in some of the periodicals of the day. An abstract of the results, and some designs for improved forms of his pump, will be found in the "Practical Mechanics' Journal."[3] His experiments were performed with much accuracy, and gave a useful effect of nearly 77 per cent. of the power applied; but the form of pump proposed was not nearly so cheap or convenient for most purposes as those subsequently introduced, and he never made any machines for actual work.

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[1] The discussion upon this Paper was taken in conjunction with that upon the preceding communication "On the Archimedean Screw for Lifting Water," and occupied portions of two evenings.

[2] *Vide* Reports of the Juries, Exhibition, 1851, p. 181, foot-note.

[3] *Vide* vol. iii., p. 4, 1850-51.

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The Author has no further information about American pumps; but if the Americans constructed good centrifugal pumps at an earlier date, they seem to have made a retrograde movement; as the only American centrifugal pump shown in the Exhibition of 1851 was found, on trial, to be inferior to one of its English competitors.

The late Mr. J. G. Appold, Assoc. Inst. C.E., was the first who raised the centrifugal pump, by its cheap and simple construction and the large duty it yielded, to a high place in public appreciation, as a new machine of great promise. As far as the Author can ascertain, the first public exhibition of his pump was at the meeting of the British Association at Birmingham, in 1849, where Mr. Appold seems to have shown a small model at work. Shortly after that date there appeared in the "Practical Mechanics' Journal" a description of a pump which he had constructed and fixed on his own premises, together with the experiments made to determine its powers, and the duty that could be obtained from it.[1] These nearly correspond in their results with those afterwards obtained by the Jury from a pump of the same size, contributed by Mr. Appold to the Exhibition of 1851, and prove the care with which the experiments were made, and that at that time the pump had been brought to a high degree of perfection.

But the invention did not obtain general notice or appreciation till it was shown at work in the Exhibition, when it became one of its marked features; and excited both surprise and admiration at the cataract of water created by a machine apparently inadequate, either in size or in power, to produce so great an effect.

A desire to ascertain the principles and proportions of the machine induced the Author to seek an interview with Mr. Appold, who invited him to see the original models and the apparatus with which the experiments were made for perfecting the machine. As far as could be judged, Mr. Appold indulged in very little theory on the subject, but he had tried almost every conceivable variation both of the form and proportions of the parts of the pump, noting in every case the effect of each, both on the work done and on the proportion of the power to the effect. By this exhaustive process, he had gradually worked his way to the construction of the machine as exhibited, which has not since been materially improved, except in mechanical arrangement.

In the Appold pump at the Exhibition of 1851, the diameter of

[1] *Vide* vol. ii., p. 230, 1849–50.

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the revolving fan was 12 inches, and the internal width was 3 inches, in two divisions of 1½ inch each. The central opening for the admission of water on each side of the fan was 6 inches in diameter, and the shape of the arms was nearly as shown in Fig. 8, Plate 9. The jury made experiments on the duty that could be got from each of the three centrifugal pumps exhibited, the experiments being conducted by their vice-chairman, General, then Colonel, Morin, the results of which are given in Table I. in the Appendix.

In these experiments the power applied to drive the Appold and the Gwynne pumps was measured by a Poncelet and Morin's dynamometer applied to the spindle of the fan, which registered correctly the power during the whole time of the experiment. The power thus estimated, therefore, does not include the amount required to overcome the friction of the engine and connecting bands for driving the pump; while in the case of the Bessemer pump, it is stated that the estimate of the power is derived from indicator diagrams, taken from the steam-engine; and therefore, it is presumed, the power required to work the engine and connections to the pump was included.

The quantity of water raised by the Appold pump was measured by taking the dimensions of an orifice, by which the water was discharged from a tank, and by measuring the head under which the discharge took place. This process is liable to some slight degree of uncertainty, owing to the motion given to the water before approaching the orifice of discharge, to the possible mixture of air, and to other circumstances; but the eminence and impartial position of General Morin no doubt secured all the accuracy the process was capable of, and it may be presumed that the results recorded are very nearly correct.

The superiority of the Appold pump, however, to the others was so great, that no possible inaccuracies of experiment can have the slightest bearing on the result, as regards a comparison between them; and, besides, such inaccuracies as are possible probably operated on all alike.

Figs. 1—7, Plate 9, represent three forms of pumps made by Messrs. Simpson & Co. from the Author's designs when he acted as manager of their works.

The principle of action is the same in all, but the varying forms are adapted to different circumstances.

In Fig. 1, *w w w* is a revolving wheel or fan, so termed from its resemblance in construction and use to the corresponding part of a ventilating or blowing fan. It is shown in vertical and in horizontal

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section, and the arrow shows the direction of revolution. The revolution of this wheel carries the water it contains along with it, and on the centrifugal force driving the water outwards, fresh water enters by the centre openings *o o*. A constant stream of water is thus created through the wheel, which, as it has no other way to escape, rises in the channel C, and is discharged. Although the general principle on which the pump acts is thus very simple, when it is desired to make a machine of suitable size and proportions to perform a certain amount of work, and to determine the power requisite to work it, the question becomes extremely complicated; and, so far as the Author is aware, the theory has not yet been investigated with such completeness and accuracy as to give results available in practice.

It has been already mentioned that Mr. Appold determined the proper forms and proportions entirely by experiment. The Author will, therefore, content himself with giving these, as determined by Mr. Appold and confirmed by his own experience, adding a few remarks on the proper shape to be given to the arms of the fan. The principal points thus determined may be given under the following heads, or rules of construction:—

1. The fan is divided, as shown in Figs. 1 and 3, by a centre plate, and there are openings in the centre of each side plate to admit the water. The arrow shows the direction of motion. The arms are curved backward, and each arm occupies one-fourth of the circumference of the fan. There are generally six arms in the fan, and the clear width of each division between the centre plate and the side plate is one-eighth of the diameter of the fan. The centre openings in the side plates for the admission of the water are fully one-half the diameter of the fan. The Author has made them from nine-sixteenths to seventeen-thirty seconds of the diameter.

The space in the case round the fan cannot be too large, and the larger the opening in the case, by which the water passes off, the better.

2. The best duty is obtained when the speed of the periphery of the fan exceeds the velocity of a falling body due to the height of the lift by from 6 feet to 8 feet per second; but the duty is not very much reduced when this excess of speed falls to 4½ feet per second, or rises to 14 feet per second, as determined by experiments with lifts varying from 5½ feet to 20 feet.

3. A fan 12 inches in diameter, and of the proportions given in Fig. 1, when working at a speed affording the maximum duty, will discharge from 1200 gallons to 1400 gallons of water per minute.

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4. If the diameter of the fan is varied, the speed of the periphery and the lift remaining the same, the delivery of water is increased or diminished directly as the square of the diameter. The proportions given in Fig. 1 are supposed to be retained.

5. Fig. 8 shows the method of describing the arms of the fan so as to give them the best shape. Each arm subtends one-fourth of the circumference of the fan. The dotted lines A A show a simple approximation for each arm, which answers very well for small pumps, while the full lines show a method of laying the arms down that is rather more accurate, and may be used for larger pumps.

6. When a centrifugal pump, properly proportioned, is worked by a steam-engine, the maximum duty that may be expected is about 55 per cent. of the indicated power in the smaller pumps, rising in the larger ones to 70 per cent.

Nothing, in connection with centrifugal pumps, has been more clearly proved, both theoretically and experimentally, than that the vanes of the fan should be curved backwards. The experiments tried by the Jury, at the Exhibition of 1851, on the Appold pump, clearly showed the advantage of this form of vane, which gave a maximum duty of ·68, while the same pump with the arms made straight, and inclined at an angle of 45° to the radius, gave a duty of only ·463, and when the vanes were made straight and radial this duty was further reduced to ·243.

The general form of the curved vane is, therefore, shown to be an element of great importance; and it is a necessary consequence of the arms being thus shaped, that the water is driven through the wheel, partly by the action of centrifugal force, and partly by the oblique pressure of the vanes on the water. The ratio which each of these forces bears to the other varies, even in the same pump, according to the proportion the speed of the pump bears to the height of the lift. When the lift is very small and the speed great, the water is expelled without having any considerable rotatory motion imparted to it. In such a case the resistance to the outward motion of the water is so small, that the oblique action of the vanes is sufficient to expel it, without giving it a speed of rotation at all approaching that of the fan. That this is the case is proved by the tabulated results of the experiment at West Hartlepool (Appendix, Table II.); for at the beginning of this experiment, the power applied to drive the pump was not sufficient to impart the speed of the fan to the volume of water actually passed through it. It will also be observed that each vane of the fan subtends an angle of 90°, and it follows that, if the water could be entirely

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prevented from receiving any motion of rotation, the fan would expel its whole contents four times in each revolution; whereas, in this experiment, it appears that during the first hour the fan expelled about 3·5 times its contents (·48 of a cubic foot) at each revolution. These considerations clearly indicate that the rotatory motion imparted to the water was very small in comparison with the velocity of the fan. The arms in these pumps were made as shown, because Mr. Appold found that they thus gave the best duty; but the trial at Hartlepool, the results of which are given in the Table, suggested to the Author that, for such purposes as emptying graving docks, it would probably be better to give the arms less obliquity, so as to discharge more water at the lower lifts, even if the maximum duty at the higher lifts should be slightly diminished. With a view of testing this, he made for the same pump a new fan with eight arms, and each arm curved backwards so as to subtend only one-eighth of the circumference instead of one-fourth as before. He believes this to have been an improvement for a pump in such a situation, but he had no opportunity of testing it as carefully as he could have wished. He could only test the comparative working of the two fans for a short time under a lift of 15 feet, with this result, that the new fan did as much work in three-quarters of an hour as the original did in one hour; but the duty given by the original fan was ·52, as compared with ·47 by the new one.

So far the result was what was expected; but more varied trials were required to ascertain completely the effect of this alteration in the fan. It would thus appear that the general contour of the arms may be considerably varied without any material deduction from the duty of the pump, but the angle which each arm subtends being fixed, it does not seem to admit of much doubt what kind of curvature should be given to them. The shape of the vanes must be such as to correspond with the path described by a particle of water in its progress from the interior to the exterior of the fan, and from this it follows that, during its passage from the interior to the exterior end of the arm, the rotatory motion of the fan must exceed that of the water by an angle equal to that subtended by the vane. Under these circumstances, it is clear that a particle of water starting at B (Fig. 8), the inner part of the vane, will make its way to E, the extremity, during the time that the fan moves, with reference to the water, over an angle of 90°. To ascertain the intermediate points of the curve, it is only necessary to divide the space in the fan occupied by the vanes into four zones of equal area, as is done by the dotted circles. The intermediate

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points of the curve will then be the points of intersection of these circles with radial lines dividing the angle of 90° into four equal divisions.

Fig. 8 shows two methods of describing the arms, when they subtend angles of 90°, by circles, so as to make them coincide nearly with the points of the curve thus found. One method is more simple than the other, but does not give quite so close an approximation to the exact curve required. With arms subtending any other angle, the process of finding the intermediate points in the curve would be exactly similar, and the radial lines would divide the angle subtended by the vanes into equal divisions.

Although these principles give the exact curvature of the arms, yet their being of the precise form thus determined is not of much practical importance, for this curvature can only be correct for a given relative speed of the water and the fan, which, in practice, can rarely be maintained with such accuracy as to make a slight deviation from the exact curve of the vanes a matter of much importance.

It has been a favourite idea with many designers of centrifugal pumps to contract the space between the sides of the fan at the circumference, so as to make the area of opening through the fan uniform, and thus preserve a uniform radial motion of the water during its whole passage through the fan. The effect of this, as well as of many other modifications, was tested experimentally by Mr. Appold, and he found that it was productive of no increase of duty.

Looking at the matter from a theoretical point of view, it would appear to be more correct to enlarge the area of the fan towards the circumference, so that the speed of the water should be gradually diminished, as much as possible, before its discharge, and thus be made to part with a portion of its *vis vivâ*.

When a mass of water is contained in a wheel like the fan of a centrifugal pump, and is placed in connection with a discharge pipe so high that no water is discharged, the water in the wheel receives a motion of rotation equal in speed to that of the wheel. Under these circumstances, the head of water supported at the discharge pipe will be that due to the pressure outwards of the centrifugal force of the revolving mass of water contained in the wheel, and may be thus calculated:

The cylindrical mass of water may be supposed to be divided into an indefinite number of small triangular sections as CDD′, Fig. 1, so small that the line DD′ may be considered straight.

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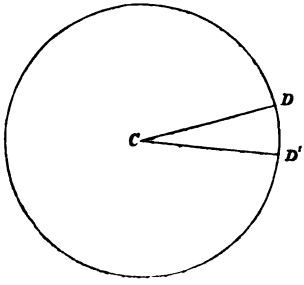
Let d = diameter of the fan.
h = head of water supported by the centrifugal force.
S = speed per second of the periphery of the fan.
t = time in seconds of one revolution of the fan.

Fig. 1.



Then the radius of the centre of gravity of any small triangular section of the cylindrical mass of water will = $\frac{1}{3}\,d$.

It will be most convenient to express the weight of this triangular section of water by the height of a parallel prism of equal weight, and having the area of its base equal to the area of the base of the triangular section, and in this way the weight of this section may be expressed by

$$\tfrac{1}{4}\,d.$$

By the ordinary formula for centrifugal force

$$h = \frac{1{\cdot}2268 \times \frac{1}{3}d \times \frac{1}{4}d}{t^2};$$

$$\text{or} \quad h = \frac{{\cdot}1022\,d^2}{t^2}, \text{ or } \frac{d}{t} = \sqrt{\frac{h}{{\cdot}1022}}.$$

$$\text{But S} = \frac{3{\cdot}1416\,d}{t}$$

$$= 3{\cdot}1416\sqrt{\frac{h}{{\cdot}1022}}$$

$$= \frac{3{\cdot}1416}{{\cdot}32}\sqrt{h} = 9{\cdot}82\,\sqrt{h}$$

$$\text{Thus S} = 9{\cdot}82\,\sqrt{h}$$

$$\text{and } h = \frac{\text{S}^2}{96{\cdot}43}.$$

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These formulæ being expressed in words give the following results:—

I. To support any given statical height of water without discharge requires a speed per second of the periphery of the fan equal to 9·82 times the square root of the given height.

II. When no water is being discharged by the fan the statical height it will support is equal to the square of the speed per second of the periphery of the fan divided by 96·43.

These rules give the theoretical results from the data assumed, but it is impossible to verify them by experiment, because the revolution of the fan, besides carrying round with it the contained water, also imparts more or less rotatory motion to the water exterior to it, and thus causes the outward pressure to be greater than the above rules give.

For fans from 1 foot to 1½ foot in diameter it is found by experiment that $S = 8\sqrt{h}$ nearly, and with large fans it approaches more nearly to the theoretical value of $9{\cdot}82\sqrt{h}$, but the Author has never found it to come quite up to this in practice.

Centrifugal pumps may be most advantageously used for small or moderate lifts, and in such cases they give as good a duty as any pumps that can be made, while they are the simplest and cheapest of all. The ordinary valve pump gives the best duty with high lifts; but for lifts of 20 feet and under, the centrifugal pump is quite equal to the valve pump; and when the lift is reduced to 4 feet or 5 feet, the centrifugal pump gives a better duty than the other.

The height of lift which it is prudent to undertake with a centrifugal pump depends somewhat on the size. The larger the pump the greater is the lift that may be undertaken by it; and, without presuming to lay down any inflexible rule, it may be stated that an 18-inch fan will work very well with a 20-feet lift, and a 3-feet fan with a 30-feet lift. On the other hand, with a 40-feet lift, the Author has found a 21-inch fan give very unsatisfactory results.

The speed required at the higher lifts, and the great power that has then to be applied, are productive of much wear, and other practical inconveniences. It must, however, be understood that these remarks apply to permanent works, where economy of power and durability are important; for, in many cases of temporary work, both these considerations are subordinate to the advantage of getting a cheap and simple machine, that will effect a temporary purpose with the smallest outlay; and in such cases centrifugal pumps may well be strained much beyond the limits here indicated.

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Another case of ordinary occurrence is where the lift required is both moderate in its maximum amount and very variable; such as in pumping drainage against tidal water, or in discharging graving-docks. In both these cases the lift is often very small, and increases irregularly to its maximum. Under such circumstances a centrifugal pump possesses peculiar advantages; for, if it is kept working at its ordinary speed, and with the employment of nearly a constant amount of engine power, it has a faculty of self-adaptation to the varying lift. Just as the lift diminishes it will discharge more water, without the employment of differential gear, or any care whatever on the part of the attendant. This quality of the pump is shown very clearly in the tables of the results of experiments made with two centrifugal pumps discharging the water from graving-docks, one at West Hartlepool, the other at Leith.[1] In each case the lift at the beginning of the operation was very small, and was subsequently increased; and in both cases the discharge, in a given time, was from two to three times as great at the beginning as at the end, although the speed of the pump and the power employed were comparatively uniform throughout. This quality of the pump may probably be made even more conspicuous than the tables indicate, by a slight alteration in the fan, as has been already suggested.

Another great advantage of centrifugal pumps is, that, having no valves, they can, without inconvenience, pass water containing large quantities of solid matter in suspension. In fact, they will discharge anything that can pass through their water passages, which, in the larger pumps, are of considerable dimensions. This quality of the pump, however, must be limited by excepting such things as ropes, yarn, long pieces of cloth, or anything that is apt in its passage to coil round the wheel. The first centrifugal pump the Author made was a small one, having a fan 15 inches in diameter, for draining a marsh. After working satisfactorily for about a week, it began to discharge less and less water, till it nearly ceased to discharge any, while at the same time it took very little steam to drive it. On examination it was found that the fan had become nearly choked with eels. These being removed and a grating with small meshes being placed between the pump and the marsh, no similar inconvenience was subsequently experienced.

It now only remains to make a few remarks on the three

[1] *Vide* Appendix: Table II.

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different forms of pump shown in Plate 9. Figs. 1 and 2 show the pump at West Hartlepool, on which the tabulated experiments were made; and, as will be seen, it is fixed under water, and with the spindle vertical. The pump is thus always charged, and can be worked without the use of any valves, which is a great advantage. But with this style of pump it is necessary to have the means of shutting the water out from the pump-well, and also of emptying it, in case of repairs being required. When a pump of this kind is properly made, however, it seldom requires repairs, as the fan and all the revolving parts under water are balanced, and produce no pressure on the bearings under water, the weight being entirely taken by a grooved bearing at the top of the upright shaft, which is quite accessible.

Figs. 3 and 4 show a pump of the kind usually employed for the smaller sizes, and where portability is required. This is a convenient machine, but it requires to have a stop-back valve fitted to the bottom of the suction-pipe, and the means of charging the pump with water before starting it. In starting a centrifugal pump, it is absolutely necessary to have it full of water to the top of the fan, and if dirty water has to be pumped, the valves on the suction pipe are apt to become leaky and inefficient.

Figs. 5, 6 and 7 show a variety of centrifugal pump invented by the Author. The fan is like one half of a fan for the other pumps, and has an opening to receive the water at one side only. It works horizontally, with its spindle vertical; and discharges the water equally all round its circumference. The water then rises in spiral courses through the vertical pipe in which the fan works. The advantages gained by this arrangement are principally these: The fan works, like that of the pump shown in Fig. 1, under water, and being therefore always charged, requires no valves; while, unlike that pump, it is so constructed that the fan and all the working parts, being fixed in conical seats, can be taken out at the top of the vertical pipe for repair, and be replaced without excluding the water from the pump-well. In fact the whole pump can be fixed in a well full of water, or be taken out under the same circumstances. Another advantage it has is this; that the fan being one-half of an ordinary one, delivers only half the quantity of water for a given diameter. This necessitates the fan, for a given amount of work, being made of larger diameter than the ordinary one, and this makes a corresponding diminution in the number of revolutions per minute required for any given lift. It is scarcely necessary to say, that anything that diminishes the speed of revolution of a centrifugal

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pump is a great practical advantage for all ordinary sizes of pumps.

Another point requires attention with reference to this form of pump. The pressure of the water on the top of the fan, over the space corresponding to the suction opening below, is clearly that due to the height of the lift, and this is unbalanced pressure, tending to increase the weight to be supported on the fan spindle. The pump shown in Figs. 5, 6, and 7, is that at Leith, on which the tabulated experiments were made. When it is working with a 20-feet lift, this unbalanced pressure would be about 3 tons, which is an undesirable addition to be carried by a spindle revolving with great speed. To remove this evil is the object of the shield S S, which is introduced above the fan. A brass lip, or rim, is formed on the top of the fan, which fits a corresponding turned rim on the shield, thus enclosing a space above the fan, which is kept in communication with the suction side, by having several holes drilled through the web of the fan. By making this enclosed space larger than the inlet opening below, not only can the unbalanced pressure be removed, but any required amount of pressure upwards may be obtained to neutralize the weight of the fan and the spindle. The weight of the moving parts in this pump is therefore supported by the water; and the friction and wear are correspondingly diminished. The Author considers that the way in which the water is taken from the fan in these pumps, equally all round, and ascending through a large pipe in spiral courses, to suit which the arms of the shield D D are made in a spiral or helical form, is the most favourable of any he has seen for economy of power. Nevertheless, he did not expect to get a better duty from these than from other pumps, because they labour under this disadvantage—that the friction of the fan in the water must be nearly the same as the friction of an ordinary fan capable of doing double the work. The practical experience he has had, however, seems to show that, as regards duty, they are the best pumps he has made. The duty shown by Table II. rises to 70 per cent. of the indicated power, and in some other experiments which the Author made with a pump of the same kind, and having only a 20½-inch fan, the duty obtained, on an average of five experiments, was 71½ per cent., with a lift varying from 20·83 feet to 23 feet.

It will be observed in both the pumps with vertical spindles that there are no footsteps at the bottom. This is essentially necessary to secure durability in shafts working at so high a speed under water, where they are exposed to the action of all sorts of grit and

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impurities carried in suspension, and liable to settle in the bearings when the pump is at rest. In the graving dock pumps the working parts are of gun-metal, and this is absolutely necessary to secure durability, especially when the pumps work in salt water. In the large pump at Leith Docks, the bearings for the fan shaft are bushed with lignum vitæ, which, the Author thinks, would be an improvement in the smaller pumps also, although he has found that two gun-metal surfaces work very well together in the circumstances in which they are here placed; that is, under water, and exposed to little or no pressure.

The principal difficulty, in securing accuracy in experiments on the performance of centrifugal pumps, generally consists in measuring correctly the water discharged. In the experiments recorded in the Appendix the water was pumped from graving docks, the contents of which were measured. Indicator diagrams were taken from the engines at frequent intervals, and at similar intervals the lift was ascertained, by measuring the difference of the height of the water in the pump well, and that outside the dock, into which the water pumped was discharged. In both cases there was more or less leakage into the docks, but no account is taken of this, and, whatever it was, it went to diminish the recorded duty of the pumps. In estimating the indicated power, the whole pressure shown by the diagrams was taken, no allowance being made for the friction of the engines. The Tables represent, as nearly as possible, the results derived from observed facts only, no theoretical allowances of any kind being made.

The communication is accompanied by a series of Diagrams, from which Plate 9 has been compiled.

APPENDIX.

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APPENDIX.

TABLE I.—REPORT of the JURY.—EXHIBITION of 1851.—Class V.[1]

Number of Experiment.	Height of Lift.	Discharge per Minute.	Ratio of Power to Effect.	Revolutions of Wheel per Minute.	Remarks.
	feet.	Gallons.			
1	8·2	2,100	·588	828	APPOLD'S PUMP.
2	9·0	1,664	·648	620	With 12-inch fan, 6-inch openings and curved arms.
3	18·8	1,164	·649	792	
4	19·4	1,236	·680	788	All these experiments were made with Poncelet and Morin's dynamometer.
5	13·4	1,248	·650	800	
6	26·2	432	·398	843	
7	27·6	681	·463	876	
					DITTO.
1	18·0	560	·398	694	With straight arms inclined at 45°.
2	18·0	736	·434	690	
					DITTO.
1	18·0	369	·232	624	Radial arms.
2	18·0	474	·243	720	
					GWYNNE'S PUMP.
1	13·8	290	·19	675	Straight parallel radial channels.
2	13·8	280	·19	920	Experiments made with dynamometer.
					BESSEMER'S PUMP.
1	3·37	832	·18	60	Radial arms and conical sides.
2	3·62	1,006	·13	71	Experiments made with McNaught's indicator.
3[2]	2·2656	..	..	40·5	
4	3·427	896	..	60	
5	3·28	846	·225	60	

[1] *Vide* "Reports of the Juries,—Exhibition of 1851," p. 182. 8vo. London, 1852.

[2] No water thrown. The water just kept suspended up to the level of lip.

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TABLE II.—RESULTS of EXPERIMENTS made with CENTRIFUGAL PUMPS at the GRAVING DOCKS at WEST HARTLEPOOL and at LEITH.

WEST HARTLEPOOL.

Time.	Revolutions of Engine per Minute.	Mean Steam Pressure on Piston in lbs. per square inch.	Indicated H.P.	Revolutions of Fan per Minute.	Speed of Periphery of Fan in Feet per Second.	Speed of ditto due to Lift, per formula $v = 8\sqrt{h}$ . . (1)	Speed of ditto ditto, per formula $v = 9{\cdot}825\sqrt{h}$. (2)	Excess of actual Speed over that given by formula . . (1)	Ditto by formula (2)	Height of Lift.	Water discharged in Cubic Feet in 15 Minutes.	Work done, H.P.	Percentage of useful Effect.
h. m.										ft. in.			
6 0	..	..	..	..	..	..	..	..	..	0 0	..	..	..
6 15	38·2	13·0	24·4	516	43·0	8·94	10·98	34·06	32·12	1 3	12,838	..	..
6 30	39·3	14·0	27·0	530	44·2	10·82	13·29	33·38	30·91	1 10	12,838	2·56	10
6 45	36·6	13·4	24·0	494	41·2	12·85	15·77	28·35	25·43	2 7	12,820	3·67	14
7 0	37·4	13·0	23·9	505	42·1	14·42	17·71	27·68	24·39	3 3	11,958	4·52	19
7 15	34·4	12·4	21·0	464	38·7	16·00	19·65	28·70	19·05	4 0	12,400	5·83	26
7 30	33·0	12·3	19·9	446	37·2	17·74	21·78	19·46	15·42	4 11	11,683	6·75	33
7 45	33·7	13·3	22·0	455	37·9	19·59	24·06	18·31	13·84	6 0	9,992	7·06	33
8 0	33·0	13·5	21·8	445	37·1	20·52	25·20	16·58	11·90	6 7	9,966	8·13	37
8 15	31·4	12·7	19·6	424	35·3	21·66	26·60	13·64	8·70	7 4	8,240	7·43	36
8 30	32·3	13·7	21·7	436	36·3	23·54	28·91	12·76	7·39	8 8	9,277	9·61	46
8 45	35·3	14·7	25·2	476	39·7	24·23	29·75	15·47	9·95	9 2	9,725	11·23	48
9 0	33·4	14·4	23·6	451	37·6	24·76	30·41	12·84	7·17	9 7	8,504	10·32	42
9 15	35·5	15·8	27·6	480	40·0	25·71	31·57	14·29	8·43	10 4	9,519	12·28	48
9 30	32·6	14·2	22·7	440	36·7	26·32	32·32	10·38	4·38	10 10	7,635	10·46	42
9 45	33·7	15·7	26·0	456	38 0	27·42	33·67	10·58	4·33	11 9	7,858	11·50	46
10 0	32·0	15·1	23·7	431	35·9	28·40	34·95	7·44	0·95	12 8	7,162	11·33	49
10 15	37·4	13·7	..	506	The supply of water is insufficient, and pump draws air.								
10 30	37·8	12·5	..	510									

LEITH.

2 30	..	..	..	..	..	..	..	..	..	0 0	..	..	..
2 40	54	24·5	91	175·5	43·65	..	..	..	..	..	..	..	..
3 0	..	..	..	..	..	..	..	..	..	4 0	93,226	12	..
3 5	56	26·0	100	182·0	45·26	..	..	..	..	..	..	..	..
3 20	55	27·5	107	178·7	44·45	..	..	..	..	..	..	..	..
3 30	..	..	..	..	..	23·54	28·91	..	..	8 8	89,058	37	..
4 0	52	30·2	116	169·0	42·03	27·71	34·03	14·32	7·99	12 0	84,912	57	49
4 30	..	..	..	..	..	..	..	..	..	15 0	80,106	70	..
4 45	54	27·0	101	175·5	43·65	..	..	..	..	..	..	..	..
5 0	53	32·5	120	172·2	42·84	32·00	39·30	12·84	3·54	16·0	69,723	70	59
5 30	..	..	..	..	..	33·94	41·68	..	..	18·0	66,939	74	..
5 45	54	27·0	98	175·5	43·65	..	..	..	..	..	..	..	..
6 0	56	27·0	102	182·0	45·26	35·32	43·38	9·94	1·89	19·6	58,146	70	69
6 30	60	21·0	81	195·0	48·50	35·78	43·94	12·72	4·56	20·0	43,296	56	70

[Mr. WILFRID AIRY

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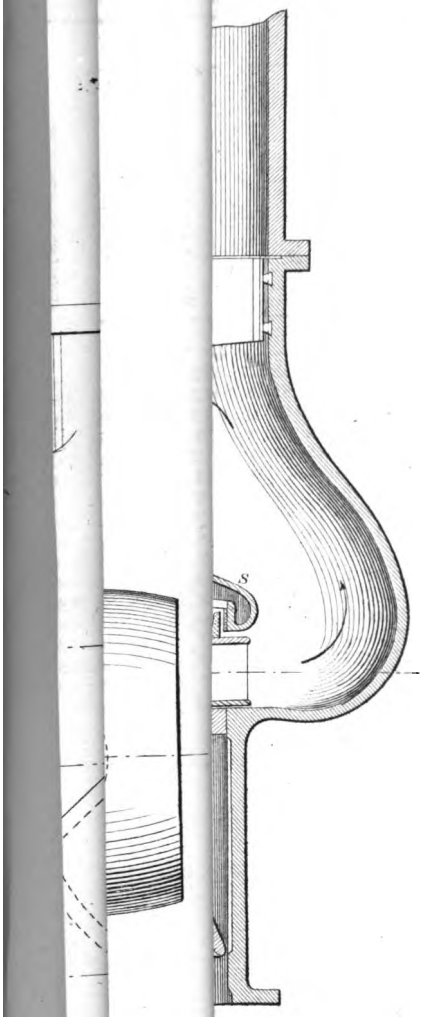
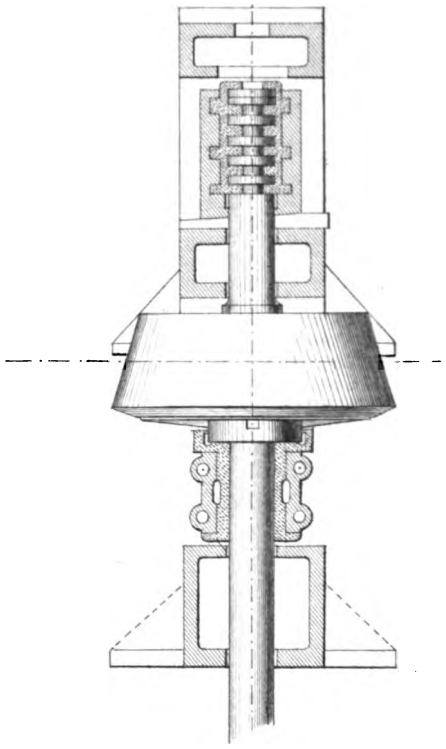


Fig. 7.



SECTION AT C. D.

Scale for Figs. 6 & 7.

1 2 3 4 5 Feet

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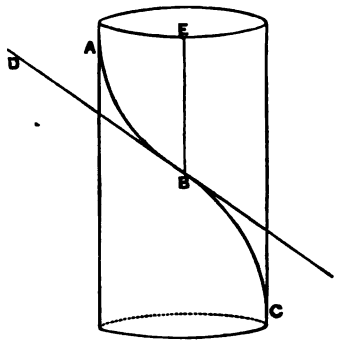
Mr. WILFRID AIRY observed, with regard to the term "spiral angle" used throughout his Paper, that in Fig. 2, A B C was a spiral line drawn on the cylindrical core; B D was a tangent to

Fig. 2.



the spiral A B C at the point B; B E was a line on the surface of the core parallel to the axis of the cylinder; and the acute angle D B E was called the "spiral angle" of the spiral line A B C.

He had used the term "spiral bend" in Table A to denote a single complete turn of the spiral chamber which held the water. The "Volume contained in a spiral bend" was, in Table A, synonymous with the "Volume delivered per revolution," in Table B, Table A having reference to screws with one thread only, while Table B referred to screws having several threads. By Table D it would be seen that at the best angle of inclination, which was 30°, the machine with developable threads would deliver half as much water again per revolution as the machine having threads at right angles to the surface of the core, which was the ordinary form of construction.

Fig. 3 (p. 42) represented a general normal section of a screw having developable threads, by a plane containing the axis of the core. The full lines showed the position in which developable threads would appear, and the dotted lines the position of threads constructed in the ordinary way. It showed the manner in which a screw having developable threads was enabled to hold more water than a screw made in the ordinary way. It was evident that the chambers formed by the developable threads would, by reason of their inclination to the core, hold more water than those formed by the dotted threads, by the amount of water lying to the left of the dotted lines. Table E showed the linear addition in feet to

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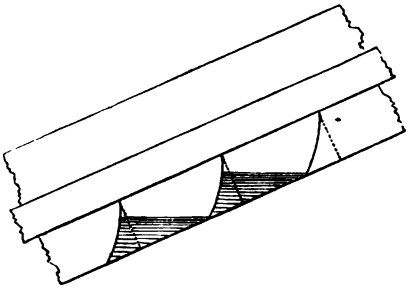
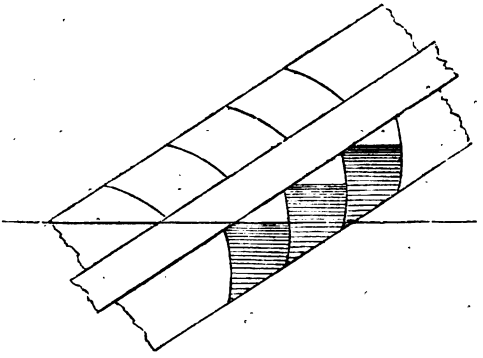
the net height of lift due to the frictional resistance of the water passing up the machines. This quantity attained a minimum in the case of the screw of 40°, which was explained thus: the

Fig. 3.



amount of friction depended on the length of the spiral chamber and the velocity of the fluid jointly. Now when the spiral was quick, the velocity of the fluid was great; and when the spiral was slow, the spiral chamber was long; and the combination of the length and velocity would necessarily produce a minimum result in the case of some intermediate spiral. Fig. 4 showed how the advantage of using several threads instead of one was

Fig. 4.



obtained. It represented a vertical section of a screw having three threads: the shaded parts were those occupied by the water, and the horizontal line showed the highest level at which the water could stand in the lowest chamber, *i.e.*, the level of the

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lip of that chamber. If then the screw had but one thread, it would deliver per revolution only as much water as lay below the horizontal line; but if it had three threads, as in Fig. 4, then the water would stand as high in each chamber as the level of the lip of that chamber, and the quantity delivered per revolution would be the total amount shaded. In the same manner it would be seen that, provided the threads themselves took up no room, the greater the number of threads, the greater would be the volume of water delivered per revolution.

He would now allude to a valuable application of the Archimedean screw, which he believed to be in a great degree novel, viz., as a means of utilizing a low fall of water, and driving mills or other machinery. The labour of turning an Archimedean screw in the ordinary way was due to the lopsided position of the water as contained in the spiral chambers, and if the screw, when full, were suddenly released, it would rapidly revolve in the backward direction. If then a stream of water were turned into the upper end of the machine, it would fill the spiral chambers in succession, and the screw would become a motor capable of driving machinery. This application of the screw had been suggested by Mr. Anderson, M. Inst. C.E., of the firm of Eastons, Amos, and Anderson, who had experimented on the model exhibited by Mr. W. Airy, to ascertain the duty of the screw when so applied. The screw was fitted with a cylindrical mouth-piece to receive the water, and it raised a weight by means of a cord coiled round the projecting upper end of the core. The net duty realized in this manner was found to be 71½ per cent., a result very nearly as good as Smeaton obtained from his best models of overshot water-wheels, and probably representing as much as 80 per cent. on a full-sized screw.[1]

With regard to the model of a scoop-wheel which he exhibited, he would remark that the ordinary form of scoop-wheel, as used in the fens, was a large wheel studded all round with

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[1] The following are the details of the experiments to ascertain the efficiency of the Archimedean screw applied as a motor: The experiments were tried on the screw model, which was 5½ feet long and 7 inches in diameter. The spiral angle of the screw was 40°: it had three threads and was placed at an angle of 30° to the horizontal. The upper end of the screw was fitted with a cylindrical mouth-piece to receive the water, and the water entered the screw from a box supplied by a tank. The screw lifted a weight by means of a cord coiled round the upper end of the core and carried over a guide pulley fixed on a tall pole. The water was measured into a circular cistern as it left the screw. The results include the friction of the pulley and other small losses, as also a loss of head amounting to

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inclined float-boards, and playing close between two vertical walls of masonry. This arrangement had several defects; the wheel raised the water higher than was necessary, the leakage past the wheel was considerable, and the wheel was apt to twist and grind against the side-walls. These defects were obviated, in the model exhibited, first, by using curved floats, which parted readily with the water; secondly, by shrouding up the floats at the sides, so as to prevent the side-leakage; and thirdly, by removing the side-walls to a distance from the wheel. There were also other advantages attending this arrangement, and the model showed certain improvements in the shape of the course to and from the wheel.

The ASTRONOMER ROYAL said it would not become him to enter into the general discussion of the Archimedean screw under the existing circumstances; but there was one point to which he might allude as relating to the simple geometry of the case, and especially with regard to the subject of developable surfaces. The term "developable surface" would probably not be found in many engineering books; but it was one with which theoretical mathematicians were familiar, and by which was meant a curved surface that could be unwrapped, laid flat, and made a plane. There were certain curved surfaces which were developable and some were not. A cone was a developable surface; a cylinder was the same: each could be unwrapped and laid flat upon a table; but a sphere was not a developable surface. Half an orange peel could not be laid flat without the edges being cracked. The surface now more immediately under consideration was a spiral; and in all the instances of the Archimedean screw he had ever seen mentioned up to this time, the idea was that the spiral possessed that sort of form which allowed it to lie upon the

about 2 inches, or 7 per cent., in getting the water into the screw. Each experiment occupied 34 revolutions of the screw.

No. of Experiment.	Weight raised 20 Feet.	Gallons of Water falling 2·4 Feet.	Number of Revolutions per Minute.	Duration of Experiment.	Work done by Weight.	Work done by Water.	Duty.
	lbs.			Seconds.			Per Cent.
1	9·86	13·1	..	..	197·2	314·4	62·7
2	9·86	12·7	43·4	47	197·2	304·8	64·7
3	9·86	11·5	37·1	55	197·2	276·0	71·5
4	9·86	14·7	63·7	32	197·2	352·8	55·9
5	9·86	13·1	58·3	35	197·2	314·4	62·7
6	9·86	12·3	31·4	65	197·2	295·2	66·8
7	12·27	14·7	46·3	44	245·4	352·8	69·5
8	12·27	14·3	30·0	68	245·4	343·2	71·2

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surface of an ordinary spiral staircase: that was the most familiar object for illustration. Take an ordinary spiral staircase, and a surface that would lie over the arrises of the steps was the form of spiral screw commonly considered as proper for an Archimedean screw. But that was not a developable surface, and not developable in a sense opposite to that of a sphere. If a spherical surface were pressed on a table the edges would crack; but if a surface like a staircase spiral were flattened upon a table the external edges would not crack, but there would be too much surface at the edges, and they would have to be nicked that the neighbouring parts might overlap each other.

The important matter now proposed was that the surface of the spiral should be made developable, and then it could be constructed of any flat material. He had brought a sample of it with him which he made some time ago in cardboard. It was a succession of rings of flat cardboard, forming several turns of a flat spiral; and of course flat iron or any other flat material could be put together in the same way. When it was screwed into a smaller circle it assumed a conical form; and if in this state it was pulled out endways the long spiral was produced, with inclined threads; and that was the spiral which it was now proposed to use in the Archimedean screw. It must be conceived that a long central core, of a diameter sufficient to occupy the central gap, was pushed through the central hole, and that there was an external cylindrical casing which was united with the external edge all round. These experiments might be made with either end of the spiral upwards; and he thought this simple geometrical point could be better illustrated by a little model than by a great deal of talk, or in any other way.

Mr. R. C. MAY inquired to what height of lift Mr. Airy considered that the Archimedean screw could be advantageously used.

Mr. W. AIRY replied that he could only give the general opinion which he had formed on the subject. He should not be inclined to use the machine for much higher lifts than 10 feet, though there was no theoretical reason why the screw should not be advantageously applied for any height of lift that its strength would allow. But if the screw were made very long it became ponderous, especially if made with many threads. For a screw whose spiral angle was 40°, the best angle of inclination was 30°; so that for a lift of 10 feet the screw must be more than 20 feet long, and such a screw of 6 feet or 7 feet in diameter, when charged with water, would need considerable strength. At the

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same time a screw possessed great strength and stiffness by reason of its structure, and could without much difficulty be further stiffened in any particular instance.

Mr. D. THOMSON wished to know whether Mr. Airy considered that his form of screw, which was developed from a flat plate, had, in addition to increased facility of construction, an advantage over other forms in point of efficiency in pumping.

Mr. W. AIRY replied that he regarded the increased facility of construction as a first advantage. But apart from this, it would be seen by Table D, that his form of screw had a great advantage in respect of the volume of water delivered per revolution. If one machine would deliver more water per revolution than another, it might be made of smaller dimensions to do the same amount of work, and would in consequence be lighter; and the friction due to the weight of the machine would be reduced.

Mr. R. C. MAY asked Mr. Thomson whether, when the centrifugal pump was applied to lift large quantities of water to moderate heights, there was any theoretical or practical disadvantage, or not, in taking the water out of the periphery of the circle at any particular place, as at the bottom or at the top? Also, whether there was any particular advantage in discharging it upwards or downwards, or at any part of the circle?

Mr. D. THOMSON said he understood the question to refer to pumps made with horizontal shafts and vertical wheels. In that case there could be no doubt as to where the water should be driven off from the case of the pump, viz., at the top. If it was taken from any other place, the top of the pump would form a reservoir for air, and so exclude the water from the wheel; otherwise he thought there was no reason why it should be taken from one place more than from another.

Mr. R. C. MAY observed that, supposing the pump was used where the water from the main flooded the pump 4 feet or 5 feet above the top of the fan, there could be no chance of air getting in. He would refer to a reservoir where, 400 yards or 500 yards from the pump, there was a head of 15 feet or 16 feet above the axis of the pump, and, allowing for friction through a main 42 inches in diameter, the top of the fan was always flooded 6 feet or 8 feet over its periphery. Under such conditions as that, he would ask whether it was a matter of importance at which point of the periphery the water should be discharged?

Mr. D. THOMSON replied that he had no experience upon that particular point, but he should suppose, under the circumstances stated, it would make no difference whatever.

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In further explanation of his Paper, he remarked that Figs. 1 and 2, Plate 9, represented the pump experimented upon at West Hartlepool, the results of which were recorded in Table II. of the Appendix. Fig. 1 was a vertical section, and Fig. 2 a plan of the same. The pump was placed in a circular well with a cast-iron plate to form a division. The water was admitted to the well on one side of this division, and the action of the pump forced it out on the other side, where it ascended in the space enclosed between the plate and the side of the well till it reached the opening by which it was discharged into the dock. Fig. 8 showed the shape of the arms of the fan. Figs. 3 and 4 showed a small portable pump of the usual description, of a size suitable for being moved about; it was driven by a belt. In the Leith pump, represented in Figs. 5—7, the fan was 4 feet 9 inches in diameter. The water was drawn up at O, and expelled in the direction shown by the arrows. The shape of the arms was shown by the full lines in Fig. 8, which also indicated the principle on which the shape of the arms was determined. He assumed that the water must of necessity be discharged at a uniform rate while the pump was working. The water did not revolve so rapidly as the pump: he had found by experiment that the pump gained upon the water; for if the water had been made to revolve at the same speed as the pump, it would have required more power to give it that speed than was exerted by the engine at the time; consequently the water could not be moving so fast as the pump. In other words, the fan was revolving through the water, not with it. On comparing different trials he had made, he thought it probable that when the pump was doing the best duty, the rotatory motion of the fan was gaining upon that of the water at the rate of about a quarter of a revolution during the time that the water passed from the inside to the outside of the fan. The area of the fan (Fig. 8) occupied by the arms or vanes was divided by circles into four equal rings or zones; and as the water must flow outwards with a uniform velocity, every particle must pass over each zone in the same time; and, as had been assumed, it would pass over the four zones in the time required by the fan to gain on the water 90° of rotatory motion. The line of motion of a particle of water, if traced on the fan, would be marked by the intersections of these circles and radial lines dividing 90° into four equal angles. These points of intersection, therefore, indicated the shape that the arms of the fan should have.

For graving docks it was, he thought, an improvement to make the arms of the pump each to subtend 45° instead of 90°. The

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maximum duty would not be so good, but the average duty would be better, especially when the pumps had to discharge a large portion of the water under a lift much less than the maximum; and this was generally the case in emptying graving docks, where the upper part of the dock was much wider than the bottom.

In both these kinds of pumps it was necessary that the fans should work under water; and thus was secured the great advantage that no valves were required. There was always a risk of valves getting choked or out of order. There was, however, this difficulty to contend with—that with pumps made like the one at Hartlepool, when repairs were required, there must be the means of excluding the water from the pump-well, so as to be able to get at it; whereas, with pumps made like the one at Leith, the whole of the working parts could be drawn out at the top, without taking the water out of the pump-well. The guide-wheel rested upon a conical seat, and the lower bearing or guide also rested upon a conical seat, and could be drawn out for repairs. This form of pump had a great advantage in that respect, and experiment showed that it gave as good duty as any other; but it was rather more costly.

Mr. G. H. Phipps said he thought the developable surface a great improvement, both as regarded facility of manufacture and as giving the Archimedean screw a power of carrying more water than when the ordinary square-threaded screw was used. There were many situations where it would be of great use; but when the water level pumped against was variable, as in tidal streams, it would not be an economical application, because of delivering at the highest water level at all times, and consequently it would be wasting power at any lower point. In this respect it did not possess the universality of the centrifugal pump, which met circumstances of varying quantity of water and elevation better than most other pumps.

He believed that the notion generally entertained as to the manner of action of the Appold pump, when it came to attract general notice, was, that the emission of water from the pump, or fan, was simply due to the preponderance of the centrifugal force of the water contained in the fan, over the pressure of the head against which it acted. Had this been so, the calculation for any required speed of discharge and quantity delivered would have been very simple; but experience had shown it to be incorrect. Under the above view all the water contained in the fan was supposed to revolve with it at its own speed; and if under these circumstances a particle of water were pushed out of the fan radially,

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by the preponderance of centrifugal force over the statical pressure of the head outside, however slowly it might move in the radial direction, its actual motion at the instant of escape would be the hypothenuse of a right-angled triangle, of which the tangential velocity and the radial velocity were the two sides; from which it would appear that, under this system, and with the speed of delivery reduced to the lowest degree possible, the power expended must always exceed that requisite to lift the water delivered to the height of the head, in the proportion of 3 to 2.

This proportion of power consumed to the effect produced corresponded, very nearly, with the best results of the experiments with the Appold pump at the Exhibition of 1851; and it would not, therefore, in itself contradict the results of the simple action of centrifugal force above referred to. Considered in another manner, however, the action of the centrifugal force and the delivery of the pump did not agree.

Take Experiment No. 4, for instance; the height due to centrifugal force at the velocity stated was only 18·8 feet, where the head pumped against was 19·4 feet, and yet with 0·6 foot too little pressure, the quantity of water delivered showed that it had to pass through the two circular eyes of the fan at a speed of 8·34 feet per second. In computing the centrifugal force of the water enclosed in the fan in this case, it had been assumed that every particle of the water, from the centre of the fan, had been put into its appropriate angular velocity; but this could scarcely be, as its tendency was to move radially until it reached the exterior of the eye; and, therefore, the centrifugal force must needs be less than the above calculation. This, although of but small difference in such a pump as that above calculated, where the eyes were 6 inches in diameter and the exterior of the fan 12 inches in diameter, came in some cases to a much larger proportion of the whole—as in one of the pumps constructed by Messrs. Easton and Amos for the river Witham drainage, where the diameter of the eyes was 4·4 feet for an outside diameter of 7 feet. It would be difficult to say, in such a case as this, what portion of the annulus of water should be taken in calculating the centrifugal force.

Great importance had been attributed by Mr. D. Thomson to the kind of curvature of the blades of the fan persistently used by the late Mr. Appold, and remarked, that a certain, but indeterminate, portion of the work of sending water through the fan was accomplished by

[1] This pump delivers 350 tons of water per minute on a lift of 6 feet. *Vide* "Engineering," vol. viii., p. 218.

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means of the inclined blades. The considerations he had just adverted to appeared to confirm this; since he had shown that the effect of centrifugal force alone was insufficient. By the adoption of this idea it was no longer necessary for every annulus of water to be moving with the same velocity as that part of the fan in which it was situated; and the direction of motion of any particle, at the moment of its passage beyond the circle of the periphery of the fan, might be nearer to the radial than to the tangential direction.

He would conclude by adverting to an instance where great tangential velocity in the water, after leaving the fan, was the point aimed at, namely, in the Ruthven water-propeller applied to the 'Waterwitch.' In this arrangement it was desired to eject the water from the ship at a high velocity, in which case the radial direction adopted for the vanes of the fan appeared correct, as the water then travelled with a speed corresponding to the periphery.

Mr. W. C. UNWIN remarked that, in the construction of his pumps, Mr. Thomson had fulfilled all the necessary conditions for securing the greatest efficiency. He had adopted the form of pump wheel introduced by Mr. Appold, and which had always proved in experiments to answer better than any other, and he had superadded to that, the clear space round the pump disc necessary for permitting the formation of, what was termed by Professor Rankine and Professor Thomson, a free vortex. The existence of that revolving mass of water round the pump disc would explain the fact noticed by Mr. Phipps, that the centrifugal pump would lift water to a greater height than that which corresponded to the centrifugal head at the circumference of the pump disc. In fact, if the chamber round the pump disc were double the diameter of the disc, and irregular eddying motion in the mass of water in it were prevented, the centrifugal head at the circumference of the chamber might be one and three quarter times the centrifugal head at the circumference of the disc.[1]

In Mr. Thomson's Paper it was stated that the efficiency of small centrifugal pumps was very much less than that of large centrifugal pumps, and he had no doubt that the efficiencies quoted really represented the results of actual experiments. But, from the theoretical point of view, there was no apparent reason why small pumps should be so much less efficient than large ones. Again, turbines were practically inverted centrifugal pumps, and he had superintended the construction of turbines varying in diameter

[1] *Vide* "A Manual of Applied Mechanics." By W. J. M. Rankine. Art. 633.

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from 6 inches to 14 feet, and had no reason for supposing that the efficiency of the small turbines was less than that of large ones. He was therefore led to suppose that, in the small centrifugal pumps which had been tried, the best proportions had not been used, and he thought that a reason could be given why, frequently, centrifugal pumps gave much less efficiency than was expected of them. He had made a careful estimate of the frictional losses of work in centrifugal pumps. Amongst these there were included the loss at the eye of the wheel; the friction in passing through the wheel; the loss from sudden change of velocity at the moment of passing from the wheel into the pump chamber; and, lastly, another source of waste of work, which, in certain cases, was much more important than all the others—that was, the friction of the sides of the pump wheel against the water in which they ran. It was well known that the work wasted by a surface rubbing against water was approximately proportional to the cube of the velocity of rubbing and to the area of the surface. If the amount of work wasted in friction by a circular disc running in water were calculated according to that law, it would be found to be proportional to the cube of the number of revolutions and to the fifth power of the diameter of the disc, and that result was confirmed by careful experiments on the friction of discs running in water. If that law were assumed and applied to calculate the frictional loss at the sides of centrifugal pumps, it would show that the choice of the diameter was restricted in the design of the pump for any given case. The diameter of the pump could not be doubled for any given case without increasing the loss from disc friction fourfold.[1] That was the reason why centrifugal pumps could not be used for high lifts. To keep down the loss from disc friction in such cases very small pumps, run at inconveniently high velocities, were required. If large pump discs, at moderate velocities, were used, the loss in disc friction in such cases became enormous. A consideration of the way in which this disc friction affected the efficiency of the pump was really the governing consideration in the design of the pump.

Now Mr. Thomson had given a rule for the proportions of the

[1] Thus, let a pump of diameter d work at N revolutions per second, then the disc friction would be $w = C N^3 d^5$ foot lbs., where C was a constant. Now, if to do the same work, there were applied a pump of a diameter $= 2d$, run at the same peripheral velocity, or making $\frac{1}{2}$ N revolutions, then the disc friction would be

$$w = C (\tfrac{1}{2} N)^3 (2d)^5 = 4 C N^3 d^5,$$

or four times as much as before.

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pump which amounted to this, that the diameter of the pump should be simply proportional to the square root of the quantity of water to be lifted. A consideration of the frictional losses increasing as the cube of the velocity would show that that rule was imperfect, and that for the same quantity of water a smaller pump ought to be used on high lifts than on low lifts. He suggested, as a good rule for the diameter of the pump,

$$d = c \sqrt{\frac{Q}{\sqrt{H}}}$$

where d = diameter of the pump in feet; Q = quantity of water to be lifted in cubic feet per minute; H = total lift in feet, including the head corresponding to the friction of pipes, &c. The value of c might be taken at 0·12 to 0·18, according to circumstances.

Mr. Thomson had given the following rule for the velocity of the pump: if v = velocity of the circumference of the pump disc in feet per second,

$$v = 8 \sqrt{H}.$$

No doubt that represented the results obtained with the particular form of pump employed by Mr. Thomson; but it might be interesting to state the results of some calculations on the influence of the curvature of the vanes on the velocity at which the pump must be run. In the formula $v = k \sqrt{H}$, the value of the constant k had been found to be—

1st. For wheels with simple radial vanes $\kappa = 10$.

2ndly. For wheels with curved vanes, the inner ends of which were so placed as to receive the water without shock, and the outer ends made an angle ϕ with the circumference of the wheel; no chamber round the wheel:

$\phi = 90°$	$\kappa = 8·3$ to 8·5
45°	8·4 to 8·8
30°	8·6 to 9·4
20°	8·8 to 10·5

3rdly. Similar wheels with a chamber round the wheel in which a free vortex was formed:

$\phi = 90°$	$\kappa = 7·1$ to 7·2
45°	7·4 to 7·7
30°	7·8 to 8·6
20°	8·3 to 9·4

He did not insist on the quantitative exactness of those figures, because theoretical calculations were necessarily imperfect. But

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they agreed very fairly with experiment, and whether exact or not they showed the precise way in which the curvature of the vanes influenced the velocity at which the pump must be run.

Mr. W. H. ALLEN directed attention to the relative commercial value of the centrifugal pump as compared with that of other kinds of pumps for raising water to elevations, say, up to 50 feet. For example, he estimated the cubical contents of the floating dock at Cartagena, recently described by Mr. Rennie,[1] at about 300,000 feet. Now, the cost of a pair of centrifugal engines and pumps combined, not including boilers, that would have lifted 300,000 cubic feet of water 25 feet high, which would be above the maximum lift of the dock, in three hours, would be about £800, whilst the cost of the reciprocating pumps actually erected, which would make four pumps with cylinders 27 inches in diameter, with engine and gearing complete, would be about £2,400; the proportion of the cost of the latter to the former was therefore as 3 to 1.

Besides, at Cartagena, there was a large quantity of piping used, as the pipes ran through both sides of the dock and were about 700 feet in length; and to each of these lengths of piping a suction or sluice valve had to be connected in each compartment. The relative cost of the pipes and connections would be £1,070 for the centrifugal pump, against £1,850 for the reciprocating pump.

Another advantage in centrifugal pumps for such a situation was, that there would be no necessity for strengthening the dock by strong beams and girders, not only to take the excessive weight, but also the thrust of the reciprocating pumps; whereas, in the case of the centrifugal pump, the whole of the weight and the work were self-contained in the pump, and the total weight was very trifling, to say nothing of the saving of freight in sending the machinery abroad. While in a case of emergency or accident the centrifugal pump could be driven at a high rate of speed, and might be even the means of saving the dock.

In Fig. 6, Plate 9, Mr. Thomson had shown the disc working horizontally. In this case the water had to go first in a vertical direction, then pass one right angle into the disc, and again another right angle into the main. He was of opinion that it would be better if the disc were turned upwards, so as to obtain two obtuse angles instead of two right angles, as the friction would be against the sides of the revolving disc in place of against the stationary casing. Mr. Thomson did not point out how the water was pre-

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxxi., p. 295.

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vented from churning after it had left the disc—a circumstance from which he had experienced difficulty in putting down pumps. Mr. Thomson acknowledged that, whether taper or parallel, the disc would do equal duty; the cost for a large dock in the latter case would be much enhanced, and there would be no advantage gained; besides an extra width of casing would be wanted; in fact, if there was any machine that required to be proportioned to itself, as well as to its work, it was the centrifugal pump. It had been shown upon good authority that water in a pipe lost velocity in passing through an opening where the pipe was enlarged, as nearly as much as it did where it was contracted; and for that reason also there was a disadvantage in making the disc parallel.

He thought it hardly fair to quote experiments made twenty years ago in preference to more recent results. The centrifugal pump was then in its infancy; the relative duties of the pumps were then as follows:—The Appold pump, 65 per cent.; the Bessemer pump, 22 per cent.; and the Gwynne pump, 19 per cent. This comparison of duty would not hold good at the present time. The data of the Gwynne pump, exhibited in competition in 1851, were not taken from the actual pump itself, but merely from the castings of an old pump that was in the Exhibition. There were merely the castings of the two cheeks and one central radial arm placed in a wooden Appold case, and upon these General Morin made the experiments the results of which were recorded. If, however, the Appold pump gave a percentage of 65 at that period, it had not much improved during the last twenty years, inasmuch as the late Mr. Zerah Colburn had ascertained the duty of the Appold pump to be 71½ per cent., against 83·2 for the Gwynne pump in 1862.

Mr. Bazalgette said he thought it would be highly desirable to have a comparison of the prime cost of various kinds of pumps, in relation to the quantity of water lifted to different heights, with the same amount of engine power. The discussion had hitherto been chiefly confined to centrifugal pumps. If it could be extended so as to embrace a comparison of the duty performed by chain pumps, plunger pumps, suction pumps, and others both portable and stationary, it would take a more generally practical shape, and be most valuable.

Mr. W. Atkinson said that, about fifteen years ago he had extensively used the Archimedean screw pumps in Spain. For lifts of about 5 feet for each pump of 14 feet in length he found them exceedingly efficient. One advantage was their extreme simplicity, having no valves, and, in fact, nothing that could get out of

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order. They acted well for muddy or sandy water, and they could be constructed with great facility in out-of-the-way places, even by an ordinary carpenter. The angle of inclination at which he found the pumps to answer best was about the same as Mr. Airy stated, viz., from 27½° to 30°; and the angle of the spiral to the axis in the pumps he had used was about 67°—a greater angle than that which Mr. Airy found to answer best. The following were the particulars of the pumps referred to:

SPIRAL PUMPS.

	Ft.	In.
Length	14	0
Internal diameter	1	6
Central core	0	6
Three threads	—	
Pitch	3	0
Inclination of thread to axis	67½°	
Inclination of pump for a 5-feet lift	27°	
Revolutions per minute	40	
Cost of labour in making in wood	£10	

Mr. G. E. EACHUS said he had known cases in which the centrifugal pump had failed from the presence of sand in the water He would therefore ask, whether it was considered applicable for lifting sewage water, or water of that nature. He thought there was great advantage in using this pump where the lift was variable.

Mr. D. THOMSON said he believed the centrifugal pump would answer very well for raising sewage, provided the lift was sufficiently small for the favourable action of the pump; but care must be taken to prevent the access of pieces of rope and things of that sort which would twist round it, as had been explained in his Paper.

The ASTRONOMER ROYAL observed, with regard to the centrifugal pump, that more reliance must be placed upon practical experiment than upon mathematics; and he would merely remark, with regard to the question insisted upon in some degree, whether the water was driven out by centrifugal force or was pushed out, that he believed there was a mixture of the two, something in the way of the ordinary propeller in steam-ships. Part of it was due, no doubt, to centrifugal force, but that was a small part, and he believed that water was chiefly shoved out in the same manner as a propeller shoved, or tended to shove, the water away from it. His opinion was, that in the present state of mathematics they did not apply to this question; and he recommended engineers not to waste their time in that way.

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He would say a few words about the scoop-wheel which was so much used in fen districts, especially in the fens of England. When he was a young man he had walked a good deal through those singular districts, and had noticed those wheels attentively at different times. About thirty years ago, being on a visit to Dr. Peacock, then Dean of Ely, he went to look at the large scoop-wheel at Prickwillow, 3 or 4 miles from that city. That was a large wheel driven by a steam-engine. He looked at it carefully, and the result of his visit was a letter to the Dean, which, though private, somehow got into circulation in the fens. The wheel was not constructed like the model wheel exhibited, but with floats nearly radial. The first thing which he remarked was the way in which the water was lifted up into the air when carried out by this scoop-wheel; but that was a circumstance of which the man who drove the engine appeared to be proud, and his praise of the apparatus was in proportion. He evidently had not taken into account that some one had to pay for the steam-power by which all the water was tossed into the air, and that that was so much waste of power. The water in the channel was lifted almost entirely over a threshold; it was lifted higher than was necessary, independently of the tossing into the air; and that was done extensively enough in hydraulic engines of various kinds. He remembered, when a boy, reading some paper of Smeaton's in which he spoke of the pumps used in pumping the bilge-water out of ships' holds; and Smeaton contended that there was loss of power from the water being pumped on to the deck while it was wanted to be got into the sea: a similar thing was done here by lifting the water over the threshold. It was evident there was an absolute loss, from lifting over the threshold and by contracting the space of delivery in any way. Suppose a stream of water running through a channel at considerable speed, and that the channel gradually contracted in one part and gradually expanded again, so that the water had to go at a greater speed through the narrow part of the channel; it might be thought at the first view that the water would be higher in the contracted part, but in reality it would be lower. The reason was obvious. To get that greater speed there must be a fall, and to change the speed from that greater speed to the slower speed which would come presently, there must be a head of water against it; and when the water from a wide channel passed through a narrow channel through which the wheel worked, and then went into a wide channel beyond, the water in that narrow channel was lower than that at which it was to be delivered, and advantage might be taken of the circumstance that the increased speed would carry it

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up higher. Having considered this matter somewhat he gave it as his opinion, that in the case of the wheel to which he referred $\frac{4}{10}$ths of the power was wasted, which might be saved by a proper arrangement of the parts. Having come to that conclusion, he stated it to the late Sir W., then Mr., Cubitt, who, on consideration, came to the same conclusion.

These were things of no small importance in connection with large tracts of land which produced crops heavier than those he had seen elsewhere, and the fertility of which depended upon a proper arrangement of these wheels. Very great expense was incurred in drainage; and good service would be rendered by engineers who would take up this matter.

Mr. A. Murray asked Mr. Airy whether he had ever considered the possibility of applying the developable screw now proposed by him to the propulsion of ships? He was not aware that any screw formed on that principle had been tried. The nearest approach to it was, perhaps, the parabolic propeller of Hodgson, about twenty-five years ago, in which the blades were bent back, as they were in Griffiths' invention, although in the latter case there was not a plane surface.

He would also ask Mr. Thomson, with respect to the hydraulic propellers of ships, whether he had noticed the drawings of Ruthven's patent as applied to the 'Waterwitch,' in which the curved blades were driven the reverse way to that usual in centrifugal pumps? and whether he considered that was the best form of pump for propelling ships? The experiments upon the propulsion of ships by such means were still very imperfect, and little or nothing was known upon this subject. Mathematical investigation had not yet been successfully applied to it; and as this, so far as it had gone, had been adverse to the principle and had failed in predicting anything like correct results for the 'Waterwitch,' he was glad to hear so high an authority as Professor Airy say, that these investigations could not be depended upon, and that practical experiments only were reliable.

Mr. J. Lee Thomas said he thought that the discussion ought not to close without some reference to the Engineers of old, who had anticipated two of the machines now under discussion. The screw, the invention of which was attributed to Archimedes, was certainly used by the Romans for the draining of mines, and in all probability its form differed but little from the Archimedean screw in use at present, except in such particulars as would necessarily arise from the substitution of iron for wood in its construction. Mr. Airy, however, by the introduction of spiral threads on the developable

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system, had clearly made a step in advance. As regarded the scoop-wheel, on the occasion of a tour in the south of Spain several years ago, he visited the Huelva district, where the Romans had mined very extensively; and some few days before he arrived at the Tharsis mine, there had been exhumed from it a scoop-wheel. It was well known before, that the Romans made use of the Archimedean screw, but he had not seen it anywhere stated that they used the scoop-wheel. The wheel alluded to was made of oak of exceedingly light scantling, and it must have been underground for nearly two thousand years. The fact was of interest in more respects than one; and it might be interesting to timber-preserving patentees to speculate how the wood had been preserved during so many centuries. The preserving medium in this case was no doubt the sulphates of iron and copper held in solution by the water of the mine. It was further to be remarked that this wheel was put together with oak pins, no nails whatever being used in its construction; and it would appear from this, that the ancients were aware that if iron nails had been used they would have been eaten away by the acid waters of the mine, and they had therefore used wooden pins. He might mention that in a work printed at Padua, in 1620, entitled "Nuevo Teatro de Machine et Edificii," there was a drawing of a wheel which took water in at the periphery and delivered it at the centre, thus raising it half the diameter of the wheel.

Mr. VIGNOLES, President, remarked that he had books in his possession three hundred years old, in which there were woodcuts of the Archimedean screw, executed in a remarkable manner.

Mr. W. AIRY observed, in reference to Mr. Appold's rule, "When a centrifugal pump, properly proportioned, is worked by a steam-engine, the maximum duty that may be expected is about 55 per cent. of the indicated power in the smaller pumps, rising in the larger ones to 70 per cent,"[1] that however valuable this observation might be as a practical rule, yet it gave no information as to the efficiency of the pump apart from the engine, because the total loss was compounded of the loss due to the friction of the engine and that due to the pump conjointly, and there was no easy method of separating those losses. It might, however, be done in the following way: First, let the engine be put in gear with the pump, and let the volume of water raised by the pump be ascertained, as also the speed of the engine, and the reading of the indicator. Secondly, let the pump be thrown

[1] *Ante*, page 30.

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out of gear with the engine, and instead of it let there be attached to the crank-shaft of the engine a dynamometer friction-break of the kind invented by M. Prony, and by successive trials obtain the work done by the engine when the conditions were the same as before, as regarded the speed of the engine and the reading of the indicator. The work of the engine thus ascertained was the net power applied to the pump independent of the friction losses of the engine, and might be compared with the work done by the pump, as measured by the volume of water lifted; and thus the true efficiency of the pump might be ascertained. He would ask Mr. Thomson whether he could give the true efficiency of the pump apart from the engine by which it was driven?

Mr. D. THOMSON said he thought Mr. Phipps must have made some mistake in his calculations,[1] because in no case did a centrifugal pump raise water so high that the statical head of the water supported by it exceeded the centrifugal force due to its velocity of rotation. If the pump-fan was revolving without delivering water the curved vanes had no effect different from radial vanes. When no water was being discharged by the pump, the water in the revolving fan was carried round with it and at the same speed, no matter what kind of vanes the fan had; and in a small pump the effect might probably be much the same, even without vanes, as the fluid friction would almost supply their place, and carry the contained water round with the fan.

He was inclined to approve of the suggestion which had been often made to alter the shape of the fan so as to make it turn upwards in place of being horizontal; it would probably be an improvement; but he had not yet had the opportunity of trying it. The alteration suggested in the shape of the fan would make the angle that the water had to go round easier, and by so much save power; but, on the other hand, it would increase the surface of the revolving wheel in the water, and so increase the friction; and which way the balance of advantage would lie could hardly be determined without trying it. He thought on the whole it would probably be an improvement. The weight of the moving parts of the pump was supported by grooved bearings

[1] A reference to the experiments cited by Mr. Phipps showed that his calculation was correct. The Author had never met with an instance of the kind in his own experience, and the experiment cited, although correctly copied from the official report, bore internal evidence of inaccuracy; as, when compared with the preceding experiment, it would appear that a diminished speed of the pump gave both an increased lift and an increased delivery of water, which was clearly impossible.—D. T.

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at the top, out of the water altogether, which was an essential point, as if there was a step or bearing at the lower end of the shaft, under water, in a pump like this, it was sure to be ground away in a very short time from the grit in the water; but the bearings as here arranged would last because there was no strain upon them, and if any grit got in it was washed out.

On the question of tapering the fan, it had been a common practice to decrease its depth at the outside; he thought unnecessarily; and as to the saving of expense, there would be none of importance, as everything would be the same, except the small difference in the width of the blades.

He had come to the conclusion, from numerous rough experiments, that for lifts below 20 feet the centrifugal pump would give quite as good duty as any other, probably better; but for lifts above 20 feet or 30 feet, the centrifugal pump could not be compared with the others.

For the propulsion of ships by pumps with any economy of power, it would require that the pumps, and the quantity of water passing through them, should be so large as to practically prohibit the plan.

All Mr. Appold's experiments had been made with a dynamometer applied to the pump-spindle, so as to show the exact power applied to the pump; and the experiments at the Exhibition of 1851 were made in the same way. These experiments were all made by indicators, being at the time the only means at command, and he had not gone into calculations to show what was the loss by the engine, and what the loss by the pump, preferring to give simply the experimental results.

With regard to Mr. Unwin's proposition, that the proportions of centrifugal pumps to their work should be varied according as the absolute sizes were larger or smaller, he thought Mr. Unwin had probably overlooked the fact, that the work done increased, when the size of the pump was increased, in the same proportion as the friction.[1]

Mr. W. AIRY, in reply on the several points raised in the course of the discussion upon his Paper, said that Mr. Phipps had made a remark concerning the volume of water that might be raised by

[1] This was confirmed by the foot-note to Mr. Unwin's observations. In the example there given, where the diameter of a pump was doubled, the velocity of the periphery remaining the same, Mr. Unwin calculated the power lost by friction to be fourfold. By the rules given in Mr. Thomson's Paper, the work done in this case would be fourfold also, so that the proportion between the friction and the work remained unchanged.

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the Archimedean screw, and had compared it unfavourably with the centrifugal pump and other machines. He, on the contrary, thought there was no loss of economy in employing large screws, rather than small ones; and the volume delivered by a screw 10 feet in diameter, and similar to the model exhibited,[1] would be 2,000 gallons per revolution. A similar screw, 3 feet in diameter, would deliver only 52 gallons per revolution; in fact, the volume delivered would be as the cube of the diameter.

With regard to the question of variable lift he could say that when the screw was laid at the best angle of effect it was immaterial to what depth the lower end was immersed. It might be wholly submerged, and yet the screw would work without loss of effect, or inconvenience of any kind. This was due to the fact that, when the screw was laid at the proper angle, there was a continuous air-duct from the top of the screw to the surface of the water; but this was only the case when the screw was laid at the angle of best effect. If the screw was laid flatter, the water in the chambers would choke the air passage, and destroy the continuity of the air-duct, and there would be great confusion between the air and the water in the screw.

It had been mentioned by Mr. Atkinson that he had adopted an angle of inclination of 30° for screws of which the spiral angle was 67°. Such an arrangement would not suit screws made with the thread he had employed, but he had no doubt the threads of the machine referred to were made in the ordinary way, by a number of small posts mortiсed into the core, and placed contiguous, so as to form a continuous thread.

He had not himself instituted experiments with regard to propellers for steam vessels, and he thought it was a question quite apart from that of the raising of water. The length of screw used was exceedingly small; and the friction of the water would be very great compared with what it was in the case of the slow-moving screws on which he had experimented.

Mr. Richard Moreland, Jun., explained, through the Secretary, the construction of screws used for the purpose of transporting grain, malt, wet hops, and other material along troughs, inclined or horizontal.

Figs. 5 and 6 represented the elevation of screw-blades square with the core, the external diameter of which was 22 inches,

[1] This screw had three threads, and was laid at an angle of 30° to the horizontal: the spiral angle was 40°.

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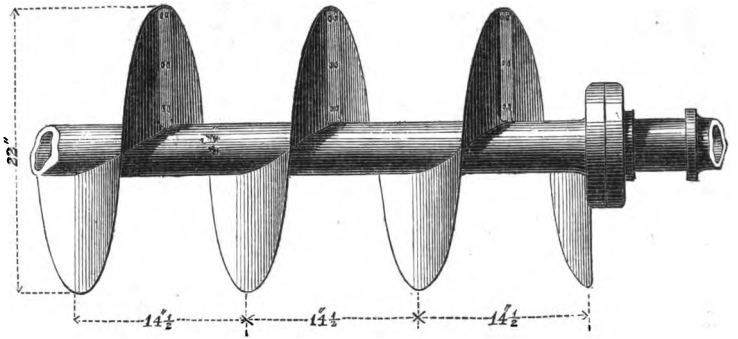
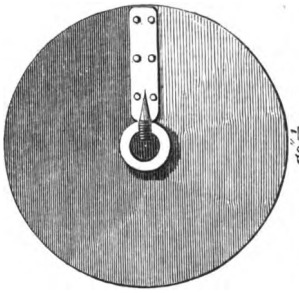
Fig. 5.



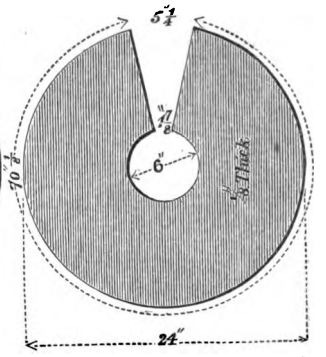
Elevation of Screw Blades, 22 inches in diameter, fixed on Cast-iron hollow shaft.

Fig. 6.



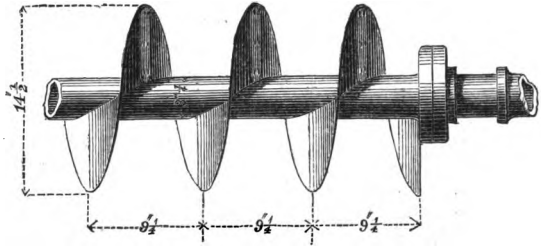
End view of Fig. 5.

Fig. 7.



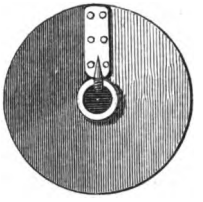
Plan of Wrought-iron Plate, as cut previously to being pressed between the dies for a Screw Blade.

Fig. 8.



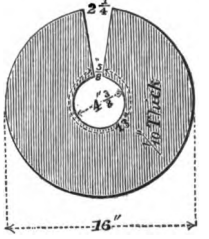
Elevation of Screw Blades, 14½ inches in diameter, fixed on Cast-iron hollow shaft.

Fig. 9.



End view of Fig. 8.

Fig. 10.



Plan of Wrought-iron Plate, as cut previously to being pressed between the dies for a Screw Blade.

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with a pitch of 14½ inches, and a core of 4½ inches. The length of the external spiral of the screw-blades was 71 inches, and of the internal spiral 19 inches. To form the screw-blades a disc was taken of the following dimensions: with a diameter of 24 inches, ⅛th of an inch in thickness, and a hole 6 inches in diameter cut in the centre; a wedge-shaped piece being cut radially, measuring 5¼ inches at the outer edge, and 1⅞ inch at the inner edge, Fig. 7.

	Inches.
External diameter 24 inches . =	75⅜
Deduct	5¼
Length of outer edge	70⅛
Internal diameter 6 inches . . =	18⅞
Deduct	1⅞
Length of inner edge . . .	17

The plate was then moderately heated and pressed between cast-iron dies formed for the purpose, when it took the form of the screw-blade as shown. In forming the blade from the disc the alterations the plate underwent were as follow: the exterior edge of the blade formed a complete spiral revolution, and the edges of the wedge-interval came in a line with and at right angles to the axis of the core: the exterior edge was extended from 70⅛ inches in the disc to 71 inches in the spiral blade; but the diameter was reduced from 24 inches in the disc to 22 inches in the spiral of the blade: the interior edge was extended from a length of 17 inches in the disc to 19 inches on the spiral of the blade; being an extension of 10½ per cent. on the length of the original line. The edges of the blades were connected by rivets to flat brass studs, which were screwed into the spindle or core, which could be either of wrought-iron tubing or hollow cast-iron shafts.

For screws 14½ inches in diameter, with a pitch of 9¼ inches, and a core of 3¼ inches, the length of the external spiral of the screw-blade was 48 inches, and of the internal spiral 13¾ inches, Figs. 8 and 9.

A plate was taken 16 inches in external diameter, $\frac{1}{10}$th of an inch in thickness, with a circular hole 4⅜ inches across in the centre; from this plate a wedge-shaped piece was cut radially, measuring 2¼ inches at the outer, and ⅝ths of an inch at the inner edge, Fig. 10.

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	Inches.
External diameter 16 inches . =	50¼
Deduct	2¼
Length of outer edge	48
Internal diameter 4⅜ inches . . =	13¾
Deduct	⅝
Length of inner edge	13¼

In forming the screw-blade from this disc the external edge remained unchanged in length; the blade formed a complete spiral revolution; the diameter was reduced from 16 inches to 14½ inches in the screw blade, and the inner edge was extended from 13 inches in the disc to 13¼ inches in the spiral of the blade, being an extension of 5¾ per cent. on the original length. In consequence of the screw blades being unable to change their form by being undevelopable, they were exceedingly rigid, and thus required to be fixed at each extremity of every blade, as shown in the Figures.

February 21, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

The discussion upon the Papers No. 1,207, "On the Archimedean Screw for lifting Water," by Mr. W. Airy, and No. 1,290, "Centrifugal Pumps," by Mr. D. Thomson, was continued throughout the Meeting.

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February 28, 1871.

CHARLES B. VIGNOLES, F.R.S., President, in the Chair.

No. 1,291.—"An Account of the Basin, the Balance Dock, and the Marine Railways, at the Austrian Naval Station at Pola, on the Adriatic."[1] By HAMILTON ELA TOWLE, of New York, U.S.A.

ABOUT the time that it became evident that the Austrian Government must eventually abandon the port of Venice as a naval station, the harbour of Pola was selected by a commission of Engineers and naval officers as a suitable one to which to transfer its *matériel*, and at which future permanent works to complete a capacious arsenal and dockyards should be constructed. Pola is situated directly south of Trieste, on the western coast of the peninsula of Istria, south-west of Fiume, and about 60 miles distant from both those ports. Venice, on the other side of the Adriatic Sea, is 80 miles distant in a north-westerly direction. The harbour of Pola is a circular bay, connected by a narrow and short outlet with the Adriatic. It is particularly favourable for the purposes of a naval station, having ample room, good anchorage, and complete natural protection.

The Austrian Government abandoned the idea of constructing excavated docks, in consequence of the difficulties experienced in making the necessary cavities in which to build the works, from the percolation of the sea water through fissures in the volcanic rock; and they determined on the American Floating Dock, Basin, and Railway system. The floating dock adopted was that known as the Balance Floating Dock (Gilbert's System); and the basin and railways were in general principle the same as those constructed at the United States Navy yards at Portsmouth, New Hampshire, and at Pensacola, Florida. These were the first dock basins with railways that had been constructed; they were contracted for in the year 1848, and active operations were commenced a few months later.

[1] The discussion upon this Paper occupied portions of two evenings, but an abstract of the whole is given consecutively.

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The function of a basin for a floating dock is to supply a place in which the dock itself may be grounded, either with a vessel upon it to undergo extensive and prolonged repairs, or to enable the vessel to be hauled out of the floating dock upon the railways; which latter operation is only required in cases where vessels are moved from the dock to land above the sea-level, or the reverse. Another use to which the basin may be put, should occasion require it, is to provide a convenient means of obtaining access to every part of the usually submerged portion of the floating dock itself, either for examination or repair. A basin to fulfil these requirements must be so constructed as to permit the dock, with or without a vessel upon it, to be floated into it, and the entrance passage to be closed by means of gates or caissons. Where the tidal rise and fall are not greater than the entire depth of the water in the basin at high tide, suitable pumping machinery must be erected.

Calculations, based upon the maximum draught, &c., of the floating-dock, determined the depth and requisite area of the basin at Pola. These dimensions were as follow:—

Width inside the enclosing walls	$211\frac{56}{100}$ feet.
Length „ „ „	$311\frac{50}{100}$ „
Depth from the top of the enclosing walls to the top of the stringers (for receiving the dock when grounded)	17 feet $1\frac{1}{4}$ inch.
Depth from the level of ordinary high water to the top of the stringers	13 feet.
Depth from the level of ordinary low water to the top of the stringers	11 „
At the highest flood tide the depth above the top of the stringers was	$14\frac{1}{2}$ „
At the lowest ebb tide the depth above the top of the stringers was	$9\frac{1}{2}$ „

The maximum difference in the hydrostatic head, inside and outside the basin, during the progress of the works, was 20 feet.

The Austrian Government preferred that the basin and railways should, if possible, be constructed at an island (called Scoglio d'Olivie) centrally situated in the harbour, and from which several vessels had already been built and launched. Having mainly in view the treacherous and honeycombed character of the rock forming the harbour, it was decided to build a foundation upon the natural bottom of the bay at the place selected. The first operation consisted in a careful survey and contouring of both the upper and the lower surfaces of the mud bottom, the mud resting upon rock below; and to the care taken in this preliminary survey, the Author attributes much of the success in

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mastering the difficulties which arose as the work progressed. The mud varied from 2 feet to 12 feet in thickness. As the rock was unfitted for holding, or even for receiving, sheet-piles, except when they might happen to strike a fissure, it was decided not to use the ordinary clay-puddle cofferdam.

It was observed that at Trieste, Fiume, and Pola, as well as at other Austrian ports, wharves and moles had been successfully constructed of a sort of concrete, bearing the name of Santorin béton; and, as these works had been found durable and permanent in exposed situations, it was determined to adopt this material, as it could be obtained at a reasonable cost, while the expense and risk attending the temporary use and final removal of an ordinary dam would be avoided. The largest blocks of Santorin béton were found in the mole at Fiume. They were 25 feet in vertical depth, and 22 feet wide at the top, at the level of low water, battering towards the bottom at the rate of 1 in 4 or 1 in 6. They rested upon loose rubble stone, consisting of ordinary quarry rubbish, deposited on a level bed along the line of the mole for a thickness of several feet. The length of these béton blocks was 50 feet; and they had been thus formed *in situ*. Plank caissons were built on shore, and were securely tied together, and then launched and floated over the spot selected. They were next loaded and sunk upon the previously-prepared bed of rubble, and the béton was deposited within them. When it had become sufficiently indurated, the planking was removed, excepting twenty-one cross timbers forming ties to the sides of the caisson, which were left embedded in each block, or section of the wall.

After definitely deciding that Santorin béton should form the substructure of the enclosing basin, and at the same time serve the auxiliary purposes of the ordinary cofferdam, it appeared that to excavate a continuous trench for the walls, to allow them to rest upon the clear surface of the rock under the mud, would involve the construction of special machinery for the greatest depths, and, after all, only secure a solid bottom at great expense, which eventually would have to sustain the weight of the walls themselves when finished. A water-tight joint seemed the one thing most required at the junction of the enclosing wall, or dam, with the bottom, for there could be no leakage or percolation through the body of the béton wall, as long as the material was not cracked by unequal settlements or strains. But the Author assumed that accidents would occur, and that they would, if not effectually provided for, prove troublesome and dangerous.

Many cross-sections of walls were considered; but finally it

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appeared that a wall of a plain rectangular section presented the greatest advantages and the least objections. In calculating the minimum thickness of walls admissible, regard was paid to the fact that the site of the basin was a greasy mud bottom, dipping towards deep water at an angle of from 2° to 10°, and therefore rendering a slip of the basin possible. Cracks and fissures were provided against by joints across the walls, at intervals of from 40 feet to 90 feet, thereby forming weak places, which being selected with respect to the irregularities of the bottom, the rock protuberances and depressions, the thickness of mud, &c., rendered the location of cracks almost a certainty. Experiments were made to ascertain the sustaining power of the mud bottom when subjected to a pressure somewhat greater than would be brought upon it from the walls and their occasional loads; and this was not found to differ much even in places where the mud varied in depth several feet. In the application of this experimental information, the prominent projections and marked depressions in the rock were found to be distant from each other not less than twice the vertical height of the wall, and this distance was assumed as the proper limit for the length of the superimposed béton blocks, as it had been previously ascertained, by experiment, that the strength of Santorin béton was ample to resist transverse fracture, if the blocks did not overhang the point of support for a greater length than their own thickness. Having determined the probable difference in the compression of the mud at the prominent projections and marked depressions, and knowing the distances of these points apart, and the vertical depth of the two blocks above, the size of the crack, or opening, that would take place between them became a known quantity. Now, if the mud above a depression were compressed 12 inches more than the mud at contiguous projections, and the length of each block were three times its depth, it was clear that the opening between the two blocks would be 4 inches at the widest part, and gradually diminish to zero at the upper surface. It was found, by this method of calculation, that no crack would be likely to exceed 6 inches in its widest part, with vertical butt joints between the adjacent blocks of béton, and the ends of the blocks marked the localities where the cracks would certainly be found. To check the passage of water through the wall, and to connect the adjoining ends of contiguous blocks of béton so as to prevent them moving upon each other laterally, a rectangular post, 18 inches by 24 inches, was inserted vertically in each of the joints, reaching from the upper surface of the blocks, through the mud, to the rock

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bottom below. The post projected 12 inches into each block of béton. These posts received the name of 'coags,' and were made of two sticks of timber, 12 inches by 12 inches, squared up and bolted together until the proper length was obtained. After smoothing off one of the sides, cross planking, 6 inches thick, was well secured by bolts and treenails, and the joints between the timbers and planking were well caulked. The largest coag joint opening was 5½ inches, after all apparent settlement ceased, and the 'coags' were found to fulfil all their intended purposes.

A brief description of the manner of constructing the sheet-piling work, or caissons, in which the blocks were formed, will now be given. A few round piles were first driven, by hand and steam pile-driving machines, from suitable floats, to which to secure mooring and hauling lines; then the side walls of the basin were staked out, by driving, 10 feet apart, two, and in some cases three, rows of round piles within the space to be occupied by the Santorin walls. The greatest care was taken to make the stage piles stand vertically, as they were to remain permanently in the walls, as well as parallel to each other, so as to permit the free movement of the béton in settling. Straight, tapering round piles were chosen for this part of the work. Cross caps, of 12-inch sawn timber, were screwed to the heads of the stage piles, and upon them, at their outer ends, longitudinal stringers, 12 inches by 12 inches, were securely bolted, the outer sides of which defined the inner and outer faces of the Santorin wall. The staging was strengthened by vertical diagonal bracing, secured to the piles, and by horizontal diagonal bracing secured to the tops of the cross caps. The vertical bracing was removed during the process of filling the caissons, as it was no longer required, the béton affording sufficient support. The greater part of the sheet-piling was of soft Italian, Styrian, and Austrian timber, 12 inches thick. Every piece was made quite straight upon the sides adjacent to, or in contact with, contiguous piles, and the piles were driven by ordinary pile-driving machines mounted upon travelling platforms, provided with double-flanged wheels resting upon iron rails, which in turn were supported upon the tops of the longitudinal stringers. To the outer side of these longitudinal stringers the upper ends of the sheet piles were secured as fast as they were driven.

As it was desirable to render the face of the béton wall uniformly smooth, it became a matter of importance to drive the sheet-piling accurately and evenly, a process which the depth of

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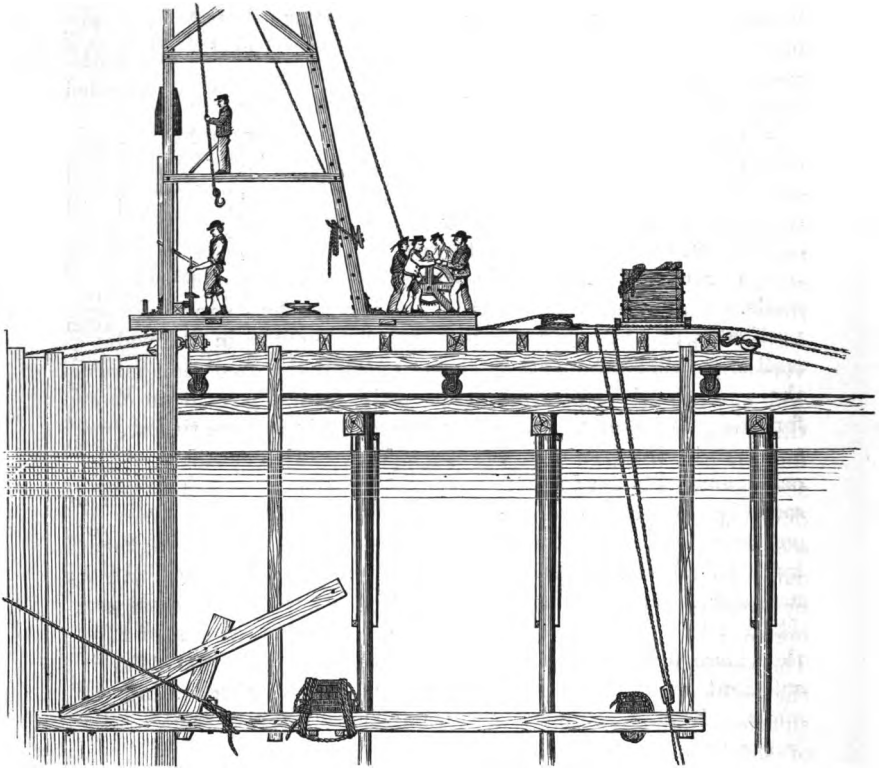
the water, the light specific gravity of the timber, and the character of the bottom, rendered somewhat difficult. To obviate this, the Author devised a machine, which for want of a better term was called a 'Spider,' and several of them were made and used about the work. It may be described as follows (Fig. 1):—

Fig. 1.



'Spider' for facilitating the driving of Piles in Deep Water.

Two sticks of tapering timber are formed, by sawing a log 35 feet long and 12 inches square, so that the larger ends are 8 inches by 12 inches, and the smaller ends about 4 inches by 12 inches. These are placed side by side horizontally, with the larger ends in the same direction, and spaced 12 inches apart, and parallel to each other, to form the side pieces. At about 8 feet from the thicker ends is fitted a block of solid oak, 30 inches thick and about 6 feet in length. A width of 9 inches on either side of the lower end of the block

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is cut away, to leave a thickness of 12 inches in the central part, which is made to project down between the parallel side pieces, and the three are securely bolted together. From its upper end, which is about 5 feet above the side pieces, the oak piece slopes toward the thicker ends, at an angle with the vertical of 30°, and is formed into a sort of throat, to receive the ends of the piles. Two inclined side pieces, or cheeks, of timber, 8 inches by 12 inches, and 17 feet long, are secured, one on each side of the oak throat-piece, near the top, and, running down, rest upon the top of the long tapering side timbers near their thicker ends, to which they are securely bolted, as well as through the throat-piece and through each other. The side cheeks and oak block now form a funnel to receive the point of a pile, and to guide it through the 12-inch opening below. Two vertical timbers, called 'hangers,' 8 inches by 12 inches, and about 20 feet in length, are inserted between the side pieces, in the rear of the oak block, and about 20 feet apart. They are each hinged at their lower ends, by stout iron bolts passing through both the side pieces and the vertical hangers. The side pieces at the rear are clamped firmly together with a distance piece between them 12 inches thick. Two ordinary tackle blocks, for receiving lateral guys, are now attached to the smaller ends of the side pieces, one on either side, and the guy ropes are rove through the blocks; a 9-inch hawser being made fast to the side pieces near the oak block. The apparatus is next weighted with ballast iron, laid across the side pieces until it will sink promptly, and it is now ready for use. Suppose a few sheet piles to be already driven, and the pile-driving machine to be in proper position to drive another pile in the line, the 'Spider' is then advanced so that the two side pieces or horns of the machine are made to pass, one on either side, and clasp the sheet piles already driven; the hawser is drawn taut until the oak throat-block presses hard against the pile last driven, and the lateral side guys, properly secured, are drawn up by the men on the platform, until the machine below is in proper line, which is readily known if the hangers are vertical. All being now adjusted, a sheet pile is raised in the piling machine, and is readily driven in line with those already down. The throat of the 'Spider' guides the point and body of the pile to its place, and the elasticity of the hawser permits a pile or two to pass through before slacking. In the result, the control of the work was so perfect, that the sheeting required, at intervals of from 20 feet to 30 feet, a special wedge-formed pile to be driven butt down, to keep the work vertical at the driving point. With a 'Spider' at least double the

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quantity of sheet piles can be driven per day in deep water, when the mud is shallow, than is possible at the same expense without such a contrivance, and the work is much better done. Between the surfaces of contiguous sheet piles was a single thread of ordinary spun-yarn or marlin, which was tacked at each end of the piles before driving. This made the joints almost water-tight, as was proved by the fact that water was often found 2 feet higher inside than outside the enclosed space.

The sides of the enclosure for one block having been completed, and the 'coags' and cross-dams having been put in, the section was bolted together by ties above the level of the béton work. Fifteen of these blocks, or sections, were formed in the entire length of the enclosing walls of the basin, one-half the number being more than 80 feet in length. At one point the béton wall is 35 feet high, and the finished wall 7½ (Vienna) feet higher; making the finished work slightly exceed 42½ feet in height, by 20 feet in thickness.

The thickness of the walls varied from 15 feet to 20 feet and 26 feet. It was decided to make the entrance for admitting the dock on the eastern side of the north end of the basin, and to adapt it for receiving an iron caisson to close it. This opening was 120 feet wide between the vertical walls, 128 feet wide at the top line of the caisson, and the caisson measured 122 feet along the bottom of the keel, each end having a batter of 3 feet in the entire height of the caisson. The method adopted in constructing the enclosing sheet-piling for the other parts of the work was slightly modified, to meet the requirements of the masonry for the caisson at this point, as well as at the south-west corner, where the pump-well was located.

Santorin béton is composed of Santorin earth—a volcanic product from the Greek island of Santorino—and common lime paste, in the proportion of 7 cubic feet of the former to 2 cubic feet of the latter, forming the hydraulic mortar; and to this is added 7 cubic feet of broken stone of the size usually employed in making concrete. The whole yields a batch containing 9 cubic feet, is made into a conical heap, and tempered by open air exposure for a period varying from one day to three days, when the heaps are ready to be used under water. Block No. 13, for example, required above 2,300 cubic yards, or 6,185 pastoni (heaps) of the prepared béton, and employed eighty-two men constantly for eighteen days to prepare the béton and put it into its place. The cross-dams at the ends of the sections were safely removed in from two to three weeks after the last béton had been deposited, when the filling of the adjoining

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section could be commenced. The average time of filling the sections was two weeks.

After the béton walls had been completed, the tie-bolts clamping the sheet piles against the sides of the blocks were gradually loosened, to enable the former to settle freely, and to compress the water from the mud below. Simultaneously, the interior rubble wall was built upon the top of the béton to a level above high water, so as to serve as a dam; and as this was finished, the material required for the superstructure of the walls was piled loosely upon the work already done, to cause the walls to complete their settlement while yet in motion.

The principal internal filling of the basin and the external sloping embankment against the sheet-piling having been proceeded with as the previous work progressed, as well as a temporary clay-puddle cofferdam, closing the intended opening for the iron caisson, pumping machinery was erected, and on the 2nd of February, 1859, the operation of emptying the basin was commenced.

In consequence of heavy winds and a maximum rise and fall of the tide, the walls were, during the pumping-out of the basin, subjected to a severe test; added to which, the water was removed from the inside of the basin to a greater depth than would occur again in its use. A slight lateral motion of the walls took place along the middle parts of the sides, by the opening of the vertical abutting joints of the sections upon the inner faces of the wall; but only one crack in the béton walls occurred worthy of mention, and this gave considerable trouble for several weeks. It was nearly vertical, and extended entirely through the pump-well wall to the re-entering angle on the outside. It made its appearance during the first twelve hours' pumping, and gradually increased in size until, upon the upper surface of the finished wall, the joints opened 3 inches. At the bottom of the wall, upon the inside of the pump-well, there was no visible opening or crack. The separating parts slightly rotated upon a ridge or ledge of rock directly below, the greater part of the opening being caused by the sinking of the corner against which no supporting embankment had been made. After all the 'coag' joints and the pump-well crack had ceased to increase in size, they were carefully cleaned out and walled-up with masonry to the depth of 1 foot from the face of the wall; and metallic tubes or pipes were inserted through the wall for conveying away any slight leakage that might escape, without washing away the fresh mortar. Where a crack or joint appeared open on the external side of the béton walls, a pad, secured to a plank or timber, was firmly braced against it, until sufficiently tight

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to prevent the escape of fine mortar or pure hydraulic cement. These openings or fissures were then filled with thin grout, injected through a tube under a head of about 10 feet above the wall, until the work was solid throughout.

The water having been exhausted from the enclosure to a depth sufficient to lay bare the earthy material previously filled in at the proper level, the work of constructing the bottom of the basin was prosecuted as rapidly as possible. This bottom consisted of thirty rows of foundation piles, capped with timber 1 foot square, embedded in béton from 6 inches below their lower surfaces to the level of their upper sides. Upon the caps was secured a platform of timber, 6 inches in thickness, upon which was laid the masonry, forming and completing nine lines of stone stringers, to receive the bottom of the dock when grounded. The stringers varied from 8 feet to 12 feet in width; and between them the finished bottom of the basin consisted of a floor of Santorin béton, 1 foot thick, on a layer of broken stone resting on the earth filling below.

The abutments, at either end of the caisson at the entrance of the basin, were built of solid masonry, well tied and bonded together. The floor or apron between the abutments was constructed of stone resting directly upon the béton wall at that part of the work, and when finished resembled in general appearance the entrance to an ordinary stone dock. The masonry superstructure, for the enclosing walls of the basin, consisted of three outside and three inside courses of headers and stretchers, having a uniform rise of 2 feet 6 inches, with a backing of common rubble masonry, the top of which was covered by flagging 9 inches thick. All the cut stone was Istrian marble, obtained from the neighbouring islands.

The space of several hundred feet between the southern end of the basin and the island (Scoglio d'Olivie) was filled in with suitable material, and two sets of railways were constructed of a length of about 700 feet. The foundation of the ways was partly of closely-driven piles and partly of masonry resting on the rock below.

The sheet piling upon the outside of the enclosing walls was cut off, by means of a circular saw specially designed for the purpose, at depths varying from 15 feet to 30 feet. The exposed ends of the piles remained as footings for the walls, and were covered with quarry rubbish to form a uniform embankment round the walls. After this was done, the interstices in the embankment, and the slight opening formed between the footing piles and the wall, were filled with common sand, such as is used

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for mortar. This was thrown from a float against the outer face of the walls, and found its way into the crevices between the solid material.

Four plain vertical cylindrical pumps of 32 inches diameter, and 36 inches stroke, with their lower ends 22 feet 6 inches below the finished wall of the basin, were erected as soon as the pump well was sufficiently advanced in construction to receive them. A pair of tubular boilers, 72 inches in diameter, and 24 feet long, each provided with a pair of furnaces communicating with a combustion chamber, furnished steam to a horizontal engine. This machinery was used for all the pumping during the construction of the basin, and was left for such occasional pumping and other work as might be required.

The caisson for closing the opening to the basin was built by the Messrs. Rennie, of London, and was found to answer its purpose perfectly. On several occasions the basin, after completion, was pumped quite free of water, and found to be almost perfectly water-tight in every part.

By the latest accounts, the basin and railways, as well as the floating dock itself, were rendering good and satisfactory service.

[Mr. HEMANS,

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Mr. HEMANS, Vice-President, said it appeared to him that the dock-wall was in fact an extended permanent cofferdam of great width filled with concrete, instead of a temporary dam filled with clay. He would ask whether the béton was let down into a dry space within the piles, whether it was dried in the atmosphere before being deposited, or whether it was let down into the water and took its set there? Also, for what reason the walls were made vertical instead of being strengthened by a batter; and further, the time required for the béton to set in the water or out of the water, what was the total cost of the wall, and the manner in which the piles were cut off by the circular saw?

Mr. A. GILES said that as the dock was only about 300 feet long, the depth of water about 20 feet, and the rise of tide only from 3 feet to 5 feet, he thought that to build a solid wall upon a substratum of mud was rather a hazardous way of getting over a difficulty, which would hardly be attempted in England. The ordinary way would be to begin by putting a cofferdam through the mud, to get the mud out, and to build upon the solid bottom. But as the mud was only from 2 feet to 12 feet thick he thought there had been a good deal of unnecessary ingenuity exercised, and it would have been more satisfactory if a temporary cofferdam had been put round what was to have been the basin, and the wall carried up from the solid rock. Under somewhat similar circumstances he had put in a bottom where there was a depth of 30 feet to 40 feet of mud to penetrate before the foundation was attained. In doing that he constructed cofferdams in pits of from 6 feet to 8 feet square, getting out the mud to the foundation and then throwing in the béton. So long as there was mud under the walls they would be liable to slip and eventually to upset the work. The dock he referred to was 420 feet long, and the cost did not exceed £63,000. If the outlay for this work for one dock had amounted to £120,000, the cost was heavy.

Mr. MURRAY said that he thought the béton surrounding wall was not so adequate a means of enclosure as a cofferdam. There could have been no difficulty in obtaining piles of sufficient length and scantling in the locality, inasmuch as they were employed to enclose the béton wall, and if there had been none in obtaining clay he considered it could have been used at a far less cost than the béton. The small thickness of the mud might have been a reason why the cofferdam system was not adopted; but there could have been no difficulty in throwing down clay in the line where the cofferdam was to be formed, and in driving the piles through it, and of obtaining thereby a firm

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foundation for the piles, even though they struck the rock. He was of opinion that a timber dam filled with clay, and, if none was to be had, even with the mud dredged from the bottom, with probably a little more breadth between the piles than usual, could have been constructed in that locality at far less cost per lineal yard than a béton wall 20 feet thick. The clay dam, if properly constructed, would have been homogeneous throughout, and no fissures need have occurred, because the bolts might have been fitted with washers hanging loosely in the clay, and they would thus prevent all possibility of leakage.

It might be urged that if the cofferdam had answered the purpose of allowing the water within the enclosure to be pumped out, yet the timber being liable to decay would cause the work in time to become defective, in fact, requiring ultimately a water-tight wall. Now assuming a cofferdam to have been constructed, it gave the means of permitting débris, or quarry rubbish, to be deposited with a slope on each side of it, which again could have been coated, if necessary, with rubble stones of a larger size to the surface of low water, when either blocks of stone, or artificial blocks of béton, could have been laid upon the deposit and upon the cofferdam carried up to any height and breadth required, thus forming a solid water-tight embankment to the enclosure.

The application of the 'spider,' for the purpose of driving the sheet piles, was quite new to him. He had often experienced difficulty in driving sheet piles in a considerable depth of water, and had been accustomed to place the main piles 10 or 12 feet apart, and to connect them with the walings under water by means of divers with helmets or the diving-bell. When these were put on, as well as the upper walings above water, there was no difficulty in driving the sheet piles perfectly tight. But in the plan here adopted the 'spider' appeared to him capable of such ready adaptation, that it was scarcely necessary to drive main piles at all, for the sheet piles might be continued one after the other with facility, which was of great importance in driving sheet piles in a considerable depth of water. In cutting off piles with a circular saw under water he had experienced much difficulty by the nipping of the saw; and the detention and trouble arising therefrom had caused him to abandon it. He preferred the common saw with hand labour from the diving-bell, with ropes attached to the heads of the piles to draw them asunder as the cuts were made.

Mr. BRAMWELL remarked that the question of cost had been put as though the work described were in itself a dock; whereas, if

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he rightly understood, it was merely the basin to contain the dock, and therefore the amount of money stated did not represent the cost of the dock and basin, but of the latter only. It had been stated that the depth of water upon the stone stringers was 18 feet, and that the basin was 311 feet long and 211 feet wide. There were, besides the dock, two short lines of railway in connection with it and the shore, each capable of receiving a ship. Taking the depth of water of 18 feet above the stone stringers, and allowing for such a floating dock as this 8 feet depth of bottom, it would leave only about 10 feet for the draught of the vessel; and if that were so this would be a very large work for docking small ships. He would be glad to hear what draught of vessel could be docked by these appliances, and whether the £125,000 represented the cost of the basin and railways, or whether it included the dock which was worked within the basin.

Mr. BAZALGETTE observed that the longer he lived the more impressed he was with the necessity of looking well to the foundations of works of this kind; he would feel disposed to exercise economy upon any portion of the structure rather than on the foundations. He thought that at Pola it would have been easy with a dredger first to have removed the mud upon which the béton had been deposited, and then to have founded it upon the solid rock. In that respect he regarded the work under consideration as an interesting experiment, and he would feel anxious to observe how far this work would, in the course of time, remain in position. He had himself been engaged recently in underpinning and deepening the foundations of two important wharves on the river Thames, which, for want of having been originally carried down to the solid ground, had given way, and the buildings were slipping into the river, after having been built and used thirty or forty years. He was not able to follow exactly the remarks of Mr. Murray, who seemed to suggest the formation of a clay-dam in addition to the piling round the béton, which would be tantamount to forming an interior cofferdam of clay requiring a solid structure behind it. He did not see that any necessity for such an addition had arisen during the construction of the work. The piles seemed to have been kept in position by being strutted and bolted together and driven through the mud, and the principal merit of this structure seemed to be its cheapness and its simplicity. The béton was apparently allowed partially to set before being thrown into the water, and by that means probably the loss of lime and consequent deterioration of the béton were prevented, which would more or less occur if the béton was deposited

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without some counteracting precaution in an unset state. The plan which he had adopted with advantage was to pass the concrete through shoots so as to prevent the lime being washed out of the mass. Mr. Ridley in his Paper upon the Thames Embankment cofferdam[1] had given a description of a circular saw for cutting off the pile heads under water, which was supposed to be the first of its kind and had been patented; and, although that was somewhat different in principle to the one used at Pola, it was interesting to know that this advantageous mode of cutting off pile heads at a great depth under water had been employed in other cases and with the same success.

Dr. W. Pole remarked that the plan of dropping concrete into its place through a great depth of water appeared open to objection, as likely to damage the composition of the material and to affect its setting power. Some years ago he had occasion to go with the late Mr. Rendel, Past-President Inst. C.E., to see the harbour works at Marseilles, where concrete was used extensively in building walls under water. The plan there adopted was very ingenious. The concrete, being first properly prepared and mixed, was placed in a box about 3 or 4 feet square, constructed in two halves, which opened on hinges at the upper part. This box was lowered through the water, and when in its place, by certain catches and chains, worked from the surface, the two halves of the box were separated, and the concrete spread itself quietly on the place it was intended to occupy, after which the empty box was drawn up and refilled. By this means the concrete was deposited without danger of any of its components being washed away, and it was found to set perfectly in the usual time.

Mr. Harrison, Vice-President, said that if he understood the Author rightly, there was no clay conveniently at hand with which to make cofferdams; therefore, though Mr. Murray had suggested the practicability of constructing cofferdams in deep water, yet to make bricks without straw, as would have been the case here, was quite out of the question. He agreed, on the whole, with previous speakers, that it was most desirable in such situations to build a sea wall on a solid foundation; but he thought the Author had given reasons for adopting the plan that had been followed, which he was not able to controvert without much greater knowledge of all the facts of the case than he possessed. He had seen the same process as that described carried out in a depth of 40 feet of water at Trieste, where the walls of the harbour were formed

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxxi., p. 3.

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of béton within a framing of sheet piling, and in some cases with guide piles only, and planks placed inside, which were afterwards removed. There was not in this country the same material at hand to employ in such works; were it otherwise he was satisfied it would be more extensively used than at present.

Mr. G. B. RENNIE said he had visited the works at Pola in 1858 during their progress, and had been conducted over them by Mr. Towle. He understood that this mode of construction was very ancient in the Adriatic, on account of the facility with which the Santorin and other materials employed were obtained; and these considerations in a great degree led to the method that had been adopted in making this dock basin. It had been estimated that the cost of a graving dock, like that at Southampton, of a length of 300 or 400 feet, was about £63,000. Now supposing the basin and railways at Pola to have been constructed at a cost of £120,000 as stated, and assuming that the dock itself cost another £100,000, for that outlay docking accommodation had been provided for five first-class vessels of from 300 feet to 400 feet long, and between 3,000 and 4,000 tons weight. That would reduce the cost per vessel to about £40,000 as against £60,000 in the ordinary graving dock. The whole docking arrangements at command, not merely the basin, must be taken into consideration. He had been informed by an Austrian officer that vessels of 2,300 tons, such as the frigate 'Schwartzenberg,' had been actually hauled ashore on several occasions, with great success, from this particular dock. It was evident from what had been done at Pola, that vessels of large size might be taken from the dock to the shore.

Mr. A. GILES remarked that Mr. Rennie's comparison of cost ought to have included the draught of water in the dock. In the Southampton dock there was a depth of 26 feet of water over the sill at high tide, so that a vessel of that draught could be put upon the blocks. The Pola dock basin represented a depth of only 14 feet, and depth formed a material element in the cost, and ought to be considered in connection with it.

Mr. G. B. RENNIE said the 'Kaiser,' a line-of-battle ship weighing over 3,500 tons, had been lifted on this dock, and her load draught-line was 27 feet. The dock was in deep water when it lifted the vessel up, and was then transported into the basin.

Mr. A. GILES said in reply that in that case it ceased to be a dock and became a pontoon, the vessel being floated over a shallow sill; although it might answer all the requirements for docking ships.

Mr. BRUNLEES mentioned that a few years since, when at Fiume,

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he noticed some newly-constructed jetties made of concrete, but in that case the concrete was in blocks placed upon a stone bank, built up from 50 feet to 57 feet below low-water level to within 20 feet of the surface. The top of the bank was levelled for a width of 47 feet, the land slope being 1 to 1 and the sea slope 2 or 3 to 1. This bank was formed of stones of all sizes, thrown in from barges and flats, and cost the Austrian Government 2*s.* per cubic yard; but the cost of the concrete blocks was 14*s.* 3*d.* per cubic yard. The concrete was formed from a compost of 6 parts of Santorin cement, 2 parts of slaked lime, and 1 part of sea-sand, which, after being well worked together, was mixed with clean small broken stone in the proportion of 1 to 1. The angles and sides were filled in with pure cement, and the blocks, when finished, were carried about 300 yards.

With regard to the foundations of the walls of the basin at Pola, he observed that if the mud under the wall was very soft, he would have no hesitation in putting the béton upon that mud, because it would sink through it and take its own position on the bottom, and carry the superstructure just as well as if the mud had been removed. If the mud was comparatively hard, then he thought it would have been better to have removed it altogether previous to putting in the blocks.

Mr. J. GRANT asked the Author whether he had made a comparative calculation of cost between the plan he had adopted and that of dredging out the foundation, as suggested by Mr. Bazalgette, and putting in the béton or concrete soon after being mixed, or in the form of blocks. Both methods had been adopted in Great Britain and in France. The plan of making the blocks, and of letting them harden for some time before putting them in place, had been extensively adopted. On the other hand, the plan of lowering the concrete through shoots, or by means of boxes or skips made to open, as mentioned by Dr. Pole, had also been followed. Each mode had its advantages. In the case of an embankment wall about to be commenced on the Thames, it had been determined not to remove a certain thickness of sand and fine gravel, which was too deep to be removed within the means at disposal, but on this stratum it had been resolved to form a foundation of two or three courses of concrete blocks, made of ballast and Portland cement in the proportion of 6 to 1. Above these the concrete would be put in after the usual manner as the tides permitted. Timber was a costly material, and he thought that if its cost could have been saved, it would have gone a long way towards making the wall wider at the base and narrower at

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the top, yet on the whole cheaper. As a general rule, concrete which had to be lowered through water must to a certain extent lose in strength. He had found that if concrete was allowed to commence setting for a certain number of hours after being mixed, and then passed through water, it would not be equal in strength to what it would be if allowed thoroughly to set out of water, or in blocks made and kept for a time above ground. Whatever strength was thus lost in the concrete was an equivalent to an increase of cost.

Mr. A. GILES observed that the Author had asserted that the Austrian Government had abandoned the idea of an excavated dock in consequence of the treacherous nature of the ground, and that the difficulty of constructing a cofferdam and getting the water out to enable them to proceed with the excavation had deterred them from taking that course. But it was stated that four pumps 32 inches in diameter, and with a stroke of 30 inches, were sufficient to keep the basin dry with a head of water against it of 20 feet. However, he was of opinion that an efficient cofferdam could have been put up more satisfactorily and at less expense than the system of construction that had been adopted. The basin was 211 feet wide and 311 feet long, which gave an extent of wall of 1,046 feet; now allowing 1,100 feet for the length of the cofferdam, he thought he could have put it down for £20 per foot. It might have been supposed in the first instance that the cofferdam was not put in because there was no clay readily at hand for the purpose, but it appeared clay was used to some extent, and it might therefore have been found for the cofferdam. If he could have put in a cofferdam for £22,000, he could have built the walls for £20,000 more, or £42,000 for the whole dock; the caisson cost little over £3,000, and these items together amounted to £45,000. There was no mention of cost in the Paper, but that must be taken into account in considering whether a work was well done and worthy of imitation; and it was said that the cost was £125,000. The Author had given 2,200 cubic yards as the quantity of prepared béton, which it was said it took eighty-two men eighteen days to prepare: according to which a man could only prepare 1½ yard of béton per day. That must have been expensive for labour if not for material. If the Author would give the cost of labour, as well as the cost of the work, it would enable an opinion to be formed whether in this country a Balance Dock might be built on a bottom of mud, or whether it would be better to go down to the solid rock.

Mr. REDMAN said he thought it was unfair to contrast this

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work with others having a similar duty to perform elsewhere. Graving docks were admirably adapted to the United Kingdom, where there was a large rise and fall of tide; but, in a comparatively tideless sea, they were to a certain extent inapplicable. He considered that the outer basin of this system of dock and slips was laid out for the protection of ships from the sea during the process of docking, under the great changes of weather that occurred in the Adriatic, and possibly also before being transferred to the slipways. In making any comparison of the kind referred to, the cost of the cubical internal capacity of the outer basin should be compared with the cost of a graving dock. This came to considerably over £1 per cubic yard of internal sectional capacity for the Pola dock—a high price. However, this system enabled two or more vessels to be docked *in transitu*, and there were slips or railways in connection with the basin, so that there was available dockage for five vessels; and taking the total cost at £125,000—which were the figures quoted—the cost was only at the rate of £25,000 per vessel.

It had been stated that a large iron-clad vessel had been docked in this basin, in which the available depth of water was from 13 to 15 feet, whereas the vessel referred to drew over 27 feet of water. This proved that the balance dock could be taken outside the basin, submerged, the vessel put on, and the receptacle pumped out and taken into dock; so that evidently the comparison with ordinary graving docks did not hold. With regard to the question of forming the foundations of the walls either on the surface of the clay or mud or on the rock, no doubt the walls might have been carried down to the rock by dredging a trench, without —as Mr. Giles proposed—putting in a cofferdam at all, or by caissons or cylinders; but looking at the limited depth of the basin, and the appliances for transferring a vessel to a higher level by means of slipways, such an outlay would undoubtedly have increased the expense of that particular portion of the work, which appeared to be about half the total cost, by about 50 per cent.

Mr. J. M. Heppel said that Mr. Rennie's explanations suggested the inquiry, Where was the necessity of any close basin at all? Because, if the pontoon, or float, or lifting dock, could be submerged, so that it could rise and fall and be floated into the basin, and allowed to settle upon the stringers, and the railway brought to such a level that the ships could be hauled away upon it, then a simple submerged platform at this level without any enclosure would have answered all the purposes required. Mr. Redman

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suggested that probably it was to protect the vessels from the effect of the waves; but if that were so, there would be no necessity to make the walls so completely water-tight, for if they were intended simply to answer as a breakwater to keep the vessel from being knocked about, a construction not water-tight would have answered the purpose just as well.

Mr. J. GRANT asked the Author —

1st. The cost per lineal foot of the basin walls complete, including timber and rubble?

2nd. The price per cubic yard of the Santorin béton?

3rd. The price per cubic foot of the timber used in the dam?

4th. The general rate of wages paid to the various classes of workmen employed?

Mr. BRAMWELL remarked that Mr. Redman had spoken of £125,000 as being the total expenditure on the apparatus which would dock the vessels, but he thought that sum did not include the cost of the floating dock, for which probably another £125,000 would have to be added. He would be glad if the Author would state the total cost.

Mr. J. N. DOUGLASS remarked that, in this case, with a depth of water of about 15 feet, and under that a bed of clay varying from 2 feet to 12 feet, and underlying that rock with a surface dipping from 2° to 10°, there were all the elements of unequal settlement and slip, which the Author had ingeniously provided for; the first by the open joint at the end of each section, and the second by the timber key in each of these joints. He thought that by the system adopted there would have been no difficulty in making a continuous béton wall founded upon the rock, and that this could have been done with but little additional cost, and without removing all the mud. One-third might have been dredged down to the rock in sections the whole width of the wall, at equal distances, and those pits when filled with béton would have formed such supports for the wall as to render it perfectly safe from immediate or future settlement. He feared, notwithstanding the open joints had been filled in, that future cracks would occur; and he would ask Mr. Towle whether, in the event of such cracks manifesting themselves again at these weak parts of the wall, he had considered the question of a remedy.

Mr. H. E. TOWLE, in reply upon the discussion, said that some misapprehension appeared to exist with regard to the functions of this dock. Looking at it as a balanced dock *per se*, if it was used without a basin it would be just as effective, and more so, than the one mentioned by Mr. Giles, built with a cofferdam, which

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could not be moved; whereas this floating dock could be taken anywhere. As to the dock itself, the floating power of each foot in depth of water was sufficient to lift a weight of 1,000 tons, and a vessel over 5,000 tons had been lifted. It was a question of locality whether an excavated dock or a basin could be built cheaply or not. In this case he had to go many miles for what clay he could get to the limited extent that he was enabled to use it.

It had been asked, Why pump the basin dry? That was for the purpose of getting access to the bottom of the dock in case of repair being required.

The experience obtained had suggested some things which might be advantageously changed or modified if the same result was to be accomplished again under similar circumstances; but the case was an unusual one, and was entirely without precedent, and demanded prompt decision and action.

It was true the Austrian engineers repeatedly came to look at the work, and to see the plans and drawings; but, without exception, till the basin was actually pumped clear of water, they contributed nothing but grave doubts of success and positive declarations that the basin would be a failure. The basin was not to sink the floating dock in at all, as that operation was performed outside, where the water was about 40 feet deep; but it was to form a level surface on which to rest the floating dock when a vessel was to be hauled out from, or returned to, the floating structure, or whenever it was desired to inspect or repair the bottom of the dock itself. And, inasmuch as the dock when tested did raise the Austrian ship of the line the 'Kaiser,' which, with its stores, actually weighed 4,223 tons, and at the same time lifted guns and other ballast to make the entire load—ship and all—up to 5,066 tons, he could not assent to the remark, that it was a very large work for docking small ships. Besides, the work would accommodate four more large vessels at the same time upon the railways. In fact, the Pola floating dock would take in and raise a vessel weighing 6,500 tons, and having a draught of 25 to 27 feet.

The problem to be solved was as follows:—A basin had to be constructed measuring somewhat over 200 feet by 300 feet on the sides inside. This basin had to be finished with a perfectly level bottom, and capable of sustaining a solid load, uniformly distributed over half of its surface, of several thousand tons. The basin enclosure had to be capable of being pumped dry, both during the period of construction of the bottom and also after the work was finished. The enclosing walls or dam were required to resist a difference of inside and outside water pressure of 20 feet during the construction

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of the work, with an outside head of 25 feet to 40 feet of water; and after it was constructed it had to resist whenever required a maximum difference of pressure, inside and outside, of about 15 feet of water, the greater head being invariably upon the outside. The site of this basin was in a harbour, whose bottom and shores were totally unreliable, and full of irregular cavities and passages of a honeycomb nature. These cavities had, on two different previous attempts to excavate for ordinary stone dock constructions, caused total failures and abandonment of the attempts, with great loss of expended money, which was due to the fact that, at a depth of about 16 feet, the rock bottoms of the excavations were crushed through by the blasting operations, and communications through the cavities directly to the sea resulted. The bottom of the harbour of Pola appeared to be of this unreliable character; but that bottom was covered with a particularly tough material, good for anchorage, from 2 feet to 12 feet in thickness. For the first 2 feet it was soft and incompact; deeper down it became more solid; and where it was 12 feet deep it was so solid throughout that a steel rod or probe 1½ inch in diameter was with difficulty forced through it by a load of about 500 lbs. weight. A stratum of 4 feet to 6 feet of this mud held piles well enough when they were properly connected together above, and less was not safe. This tenacious mud would, exclusive of the upper surface, sustain without yielding a load of several tons per square foot. It lay all over the site of the basin, in one unbroken tough sheet, covering the rock on which it rested, and covering at the same time all cracks and subterranean and submarine passages and water-ways, which were known to pervade the earth's crust in the vicinity of Pola.

Under these circumstances, and with a site exposed to sudden and violent gales of wind, the question arose, how to build a water-tight basin? Was it safe to remove the water-tight homogeneous and compact natural plaster, or stratum of mud, which in the absence of tidal currents had settled upon the doubtful parts of the bottom? And if so, should it be removed in a trench wider than the base of the wall before forming, and then in some way anchoring, the wooden moulds for the béton sections? Or should it be removed within the wall space bounded by the lines of sheet piling after it had been driven in? If removed before the wood-work was put in position, there would have been a depth of 25 feet to 50 feet of water, and wooden caissons (sheet piling being out of the question) of at least this height, must have been constructed of considerable length, and without internal struts and ties, and

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at the same time of sufficient strength to be launched from the shore and handled about till adjusted and securely anchored in place. These would have required great accuracy in their construction, that their lower edges might conform to the irregular surface of the rock bottom. Some of them, with the thickness of walls which were adopted, would have been simply huge boxes without top or bottom, and deficient in bracing, and would have had a length of 75 or 80 feet, a height of 40 to 50 feet, and a horizontal thickness of 20 feet, and one such would have taken for its construction about 15,000 cubic feet of timber, which of the kind available at Pola would have required 225 tons of ballast to sink it.

Again, admitting this system of large portable moulds or caissons to be quite possible of themselves, though a caisson of 25 feet in depth had been the largest hitherto achieved, he had seen in Pola harbour the frames and caissons of similar moulds of only 12 to 15 feet in depth wrecked in much less exposed situations, and destroyed in a moderate gale of only a few hours' duration. The enormous cost of this plan would be apparent, without figures in detail, when it was considered that it involved expensive dredging, costly caissons, a greatly increased quantity of béton, to replace the excavated mud, and the difficulties of constructing and maintaining the work intact during its progress. He admitted however without reserve the correctness of the general principle of starting foundation walls from the solid rock, provided it could be carried out practically without counterbalancing attendant evils. Had a trench been excavated for the wall of the basin there would have been risk of uncovering, on either side of the line of the wall, passage ways through the honeycombed rock, such as caused the failure and abandonment of the works previously attempted for an ordinary excavated dock; and he thought it imprudent to incur this risk when a cheaper and safer method was available for constructing the wall. Mr. Murray seemed to have considered the problem in hand as one more particularly raising the question of the superiority of temporary cofferdams; but the problem was to get a permanent water-tight masonry basin, which could be laid dry at any time under a possible maximum head of about 15 feet of water.

Béton was the cheapest material as well as the best for the permanent structure; and even if its cost was two or three times greater than that of a clay cofferdam, the latter was not required at all by his method of construction. The object was to build the basin walls as economically as possible with or without cofferdams. To have driven a cofferdam would have been perfectly practicable; but while it was serving its purpose as a dam, the timbers

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in it could not have been used for the timber-work in the bottom of the basin, as was the case with the sheet piling which was cut off after the walls had hardened. And then, again, the quantity of timber required for a temporary clay cofferdam, and that required for the bottom and other parts added to it, would, to say nothing of the difficulty of getting proper clay, have made an exterior clay cofferdam excessively costly, even with the thinnest possible permanent wall that would have served the purpose. The cofferdam must have been very thick and ponderous if the water was taken out to the bottom rock, which in the northern part was about 50 feet below the water level outside; he knew of no such dam in such an exposed situation, and it would have been very expensive. The rotative forces on such a dam, the earth being removed from the inside, to be in equilibrium with the mass acted upon by gravity alone, would have required a thickness of between 19 and 20 feet of wall; and the forces tending to slide the dam horizontally along the rock on which it rested, with a coefficient of friction of 0·25, would require to keep them in equilibrium a thickness of wall of more than 48 feet, and in the deeper water still more. If the inside wall, with such an outside cofferdam, only went down and was made water-tight with the shallow bottom of the basin, then when the exterior dam was removed and the basin pumped dry inside, as it had been repeatedly, the basin itself, if it held together, would have floated.

Calculations relating to the stability of the Basin Walls at Pola.

Problem 1. Required the thickness of a rectangular wall or dam, 35 feet high, of Santorin béton, to sustain a pressure from sea-water, upon one side having a head of 34 feet, and upon the other side a head of $(34 - 17\frac{1}{2}) = 16\frac{1}{2}$ feet.

Sea-water weighed 64 lbs. per cubic foot.
Santorin „ 120 „ „

Let h = height of the wall or dam, and l = length.
h_1 = „ water outside.
h_2 = „ „ inside.
w = weight of the fluid per cubic foot.
w_1 = „ material in the dam per cubic foot.
x = thickness of the dam.

The dimensions are in feet, and the weights in lbs.

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For the (outside) hydrostatic pressure P_1

The area pressed . . . . . $= h_1 l$;
Centre of gravity below surface . . . . $= \frac{1}{2} h_1$;
Density of fluid . . . . . $= w$;

[1] . . . . . $\therefore P_1 = \frac{1}{2} l h_1^2 w$;

Centre of pressure on outside above (a) axis $= \frac{1}{3} h_1$;

[2] . . . . $\therefore$ Statical moment of $P_1 = \frac{1}{6} l h_1^3 w$.

Similarly, for the (inside) hydrostatic pressure P

[3] . . . . . $P_2 = \frac{1}{2} l h_2^2 w$;

and in reference to axis (a) the

[4] . . . . Statical moment of $P_2 = \frac{1}{6} l h_2^3 w$.

Let R = the resultant of the pressures $= P_1 - P_2$; this gave for R

$$P_1 - P_2 = \frac{1}{2} l h_1^2 w - \frac{1}{2} l h_2^2 w, \text{ and}$$

[5] . . . . $\therefore R = \frac{1}{2} l w (h_1^2 - h_2^2)$.

Let z = the distance this resultant acted above the axis of rotation (a), then its statical moment was

[6] . . . . . $R z = \frac{1}{2} l w z (h_1^2 - h_2^2)$.

From equations [2] and [4] were obtained the following expressions for the value of this statical moment:

$$R z = \frac{1}{6} l h_1^3 w - \frac{1}{6} l h_2^3 w; \text{ reducing}$$

[7] . . . . $R z = \frac{1}{6} l w (h_1^3 - h_2^3)$:

and substituting the value of R in equation [5] and dividing, there was obtained

[8] . . . . . $z = \frac{1}{3} \frac{(h_1^3 - h_2^3)}{(h_1^2 - h_2^2)}$.

The sum of the statical moments of the water pressures taken with respect to (a) the axis was as above ascertained, equation [7],

Formula . . . . $R z = \frac{1}{6} l w (h_1^3 - h_2^3)$,

tending to turn the wall inward. To resist this, there was the weight of the wall or dam itself, and the pressure of the filling of earth upon the inside of the basin, which latter for the present would be neglected.

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OF THE RESISTANCE OF THE WALL OR DAM.

The volume of the wall . . . vol. $= x\,h\,l$;
„ weight „ „ . . . $W = x\,h\,l\,w_1$;
Its statical moment about (a) was

[9] . . . . . $W \times \frac{1}{2}x = \frac{1}{2}x^2\,h\,l\,w_1$;

and for equilibrium,

[10] . . . . . $\frac{1}{2}W\,x = R\,z$.

Now substituting in [10] the values from equations [7] and [9], and afterwards reducing and solving,

$$\frac{1}{2}x^2\,h\,w = \frac{1}{6}w\,(h_1^3 - h_2^3),$$
$$x^2\,h\,w_1 = \frac{1}{3}(h_1^3 - h_2^3)\,w,$$

the general formula for thickness was obtained,

[11] . . . . . $x = \sqrt{\frac{1}{3}w\frac{h_1^3 - h_2^3}{h\,w_1}}$.

Substituting the numerical values of Problem 1,

$$x = 13\tfrac{3}{10}.$$

Problem 2. To find the thickness (x') a cofferdam must have to stand upon the rock bottom, without either water or earth inside, taking the material at 120 lbs. per cubic foot for the clay dam:

The cofferdam had a height $h = 35 + 12 = 47$.
„ head of water was . . $h_1 = 34 + 12 = 46$.
$h_2 = 0$ $w = 64$ $w_1 = 120$.

Substituting in the general formula [11] for thickness,

$$x = \sqrt{\frac{1}{3}w\frac{h_1^3 - h_2^3}{h\,w_1}} = \sqrt{\frac{1}{3} \times 64 \cdot \frac{46^3}{47 \times 120}};$$
$$x' = 19{\cdot}1833;$$

therefore for equilibrium against rotation the thickness was nearly 20 feet.

For equilibrium against sliding from the water pressure, which was over 30 tons per lineal foot of the dam, with a coefficient of friction of 0·25, the thickness would have to be more than 48 feet. Of course when the water was deeper, as at the north-west corner of the basin, the thickness in either case must be correspondingly greater.

The plan for removing the mud, instead of building upon it,

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by dredging to the rock bottom inside the timbers forming the sheet-piling walls received much attention. No doubt it was the most certain way to found the walls upon a solid bottom, but it presented the disadvantage of greater cost. The wood-work, sheet-piling, &c., would have required bracing and strengthening, and also loading down by ballast, if the interior mud supports to the piles had been removed. The dredging itself in the narrow space of from 15 to 20 feet, encumbered by the upper cross frame-work and bracing and the staging piles, would have been troublesome and expensive. The walls if carried down on an average of 6 or 8 feet deeper than the water would have required 20 per cent. more materials in the Santorin béton parts, and their cost would have been increased in this proportion.

It would, he thought, be admitted that so long as mud, or any other material, under a wall could not escape, even if fluid, the wall could not go down by settling vertically. And if such mud in a tight enclosure was so powerfully pressed that its fluid parts disappeared, as the water in a submerged sponge would do in a press acting under water, and left the solid material, which never again would have to sustain as much pressure as it had already received, then such a wall, so far as its mere weight was concerned, had an adequate support. Suppose a great amount of lateral friction were now brought about, by embedding this wall upon either side in such wood and earth work as the Santorin walls at Pola had provided for them, additional vertical as well as lateral support would be given to the wall; in the same way that an ordinary pile got a great share of its sustaining power from side friction. This friction was increased at Pola by the use of fine sand, which was carefully thrown against the outer sides of the walls, and which by constantly insinuating its particles under the gentle action of the water and of gravity continued to give increasing stability to the wall.

Reference had been made to jetties in France constructed of béton blocks, but these jetties would allow the water to flow from one side to the other even at a moderate depth. He had made comparative valuations of cost between the method adopted and that including dredging out, and the economy was in favour of the adopted plan.

Respecting the cakes of concrete referred to by Mr. Grant, if he meant small masses of a few cubic metres such as were used at Marseilles, Caen, and some other places for breakwater works, no estimate was made, for the reason that no plan occurred to him in which such cakes could be of effective service, or at all compete

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with the large cakes containing 1,500 to over 2,000 cubic metres. That there would have been less material in a wall with a wide base and narrow top was true; but in the present case it was desired to hold the blocks under perfect control during the progress of settling. If the sides of the enclosing wood-work had been such as to allow the inverted wedge form to be given to the cross section of the blocks, any settlement of them would have caused the wall to drop away from the sides, and be free to yield to any forces acting to throw it out of the desired line of the wall and also impair the foundation. Besides, a width of from 15 to 20 feet was not too much on the top surface of the walls, and a greater width below would have prevented rather than facilitated the immediate subsidence of the wall to its lowest point, which was promoted by top-loading the walls, and removing the water from one side to a greater extent than would occur again.

The cost of Santorin béton from 1857 to 1860, at Pola, including all materials and labour when deposited in a trench, was about 80 Austrian florins per cubic klafter, or approximately 10 florins per cubic yard. The cost per cubic foot of pine timber in the dam, of 1 foot by 1 foot, was, at 3 florins 33 kreutzers per lineal klafter, about $\frac{2}{3}$ths of a florin. Round pine piles cost about $\frac{1}{3}$rd florin per lineal foot; and very large square oak timber cost 2 florins per cubic foot.

The wages of a mason. .	were	$1\frac{1}{2}$ to 2	florins	per day.
„ „ mortar-maker	„	$1\frac{1}{4}$	„	„
„ „ labourer	„	1 to $1\frac{1}{4}$	„	„
„ „ carpenter	„	$1\frac{1}{2}$ to $2\frac{1}{2}$	„	„
And the hire of a cart and two horses . .		5	„	„

In May, 1859, a cofferdam was made in the ordinary way at Pola, after the basin had been pumped out, on the other side of the same island, where the greatest depth of water was 15 feet, but it failed.

Santorin béton had invariably been spoken of as if it were the same as ordinary concrete or béton made with Portland or Roman cement, or of some of the various kinds of hydraulic cement, but this was an erroneous idea. When newly made it was not fit for use until it had been exposed to the open air for a period of from two to six days, in heaps of about $\frac{1}{3}$rd of a cubic yard each. This was the size of the batches, compounded of 7 cubic feet of Santorin earth, and 2 cubic feet of lime paste, which formed Santorin mortar of remarkable strength, to which was added 7 cubic feet of broken stone. These together made

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9 cubic feet of Santorin béton. When this béton was to be used in water 10 or 15 feet deep, the heaps were broken with a spade or pick into pieces convenient for shovelling into barrows, coarse and fine together, and simply 'dumped' into the moulds in horizontal layers uniformly distributed, which was ascertained by the foreman in charge continually measuring with a rod suitably marked. In depths not exceeding about 20 feet, the cementing materials were not washed away from the fragments of béton and pieces of broken stone, but reached their positions in the wall in proper condition to unite firmly with the rest; but in the case of the deep blocks between 25 and 40 feet in vertical thickness, he found it advisable to guard against this wasting action of the water during the fall of the béton to the bottom. Baskets, spouts, tubes of canvas, boxes, such as had been used at various works for depositing concrete, which opened in two parts, and cloth bags in which to lower the béton by ropes, and then untie and deposit the contents, were duly considered. But the unusual depth of water and the great quantity of material to be let down—some of the single blocks contained 2,200 cubic yards of béton—and the expedition required to complete one block in a reasonable time, induced him to seek a more expeditious way of procedure, and in the end he simply deposited the heaps of 9 cubic feet each in a solid unbroken mass, in the deep water; there the lumps were crushed by iron rammers fastened to the ends of long rods for handles; this expedient overcame all difficulties, and was at the same time the most economical.

The Santorin earth, damp and loose as usually measured from the heap, weighed 50 lbs. per cubic foot. A cubic foot of Santorin earth absorbed 30 lbs. of water, and the simple addition of the water caused the earth to shrink to two-thirds its original volume. It was very similar to puzzuolana and was of a volcanic nature; a large percentage was in pieces of about the size of a pea, diminishing down to sand. Mixed with common lime it made a very hard and strong material; and he had seen entire arches made of it, and it was capable of being worked like stone.

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March 7, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

The following Candidates were balloted for and duly elected:—EDWARD DANGERFIELD, JOHN IMRAY, M.A., and JOHN PENDLEBURY, as Members; WILLIAM BARBER, JAMES ALSTON CARFRAE, Stud. Inst. C.E., JOSEPH JAMES CURLING, Lieut. R.E., FRANCIS DAWSON, JOSEPH EMERSON DOWSON, JAMES ELDRIDGE, THOMAS ROBERT GAINSFORD, Stud. Inst. C.E., JOHN EDWARD HILTON, WILLIAM ROBERT HOUGHTON, Lieut.-Col. B.S.C., GEORGE HENRY LIST, JAMES JOSEPH MEAGHER, EDWARD ROSENBUSCH, WILLIAM SHIELD, Stud. Inst. C.E., LOUIS STERNE, and JOHN EVELYN WILLIAMS, as Associates.

It was announced that the following Candidates, having been duly recommended, had been admitted by the Council, under the provisions of Sect. IV. of the Bye-Laws, as Students of the Institution:—THOMAS HOLMES BLAKESLEY, OSWALD BROWN, OLIVER FRY, EDMUND PONTIFEX HARRISON, WILLIAM RUSSELL, FREDERICK VALENTINE, and JAMES WILLIAM WARDLE.

The discussion upon Mr. Towle's Paper on "The Pola Basin, Floating Dock and Marine Railways" was continued throughout the evening.

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March 14, 1871.

CHARLES B. VIGNOLES, F.R.S., President, in the Chair.

No. 1,296.—"On Phonic Coast Fog-Signals." By ALEXANDER BEAZELEY, M. Inst. C.E.

FEW, if any, portions of the coasts of these islands are exempt from occasional fogs and mists,[1] which render a landfall precarious by day, and which are not unfrequently so dense that by night even the powerful beams of the electric light fail to penetrate them to any but the most moderate distance. It is, therefore, not a little surprising that, except of late years, the subject of fog-signals for the guidance and warning of the mariner under such circumstances should have received so little attention; and that, with the exception of a few inventions of recent date, the knowledge of the present day respecting the means of producing and projecting to a long range the sounds to be employed, is but little in advance of what it was a century ago. The literature of the subject is extremely meagre: here and there, scattered among the scientific records of this and other countries, an occasional notice or a brief suggestion may be met with; but of systematic research and experiment, and their proper offspring, practical result, there are few traces. The purpose of the Paper is to endeavour to show what has hitherto been done, and to place before the Institution a brief account of the present state of information upon the subject.

Before entering upon details, it may be well to notice a misconception which is generally prevalent, and which seems to have received some degree of sanction from no less an authority than M. Reynaud, Director-General of the French Lighthouse Department,[2] viz., that great power and long range of sound are by

[1] Meteorologists discriminate between fog and mist as distinctly different atmospheric phenomena. In the present Paper the general term 'fog' is applied to all obscuring media suspended in the atmosphere at or near the surface of the sea.—A. B.

[2] "On n'a pas jugé que, dans l'état actuel des choses, la navigation de notre littoral eût assez d'intérêt à percevoir les sons à grande distance pour motiver les dépenses que nécessiteraient l'établissement et l'entretien de ces machines" [pour comprimer de l'air à plusieurs atmosphères].

"Il n'a pas été fait d'expériences par des vents violents; mais on sait que, quand ils sont debout, ils arrêtent les sons les plus forts. On sait aussi d'ailleurs

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no means essential to fog-signals, inasmuch as fogs are usually found to occur in comparatively calm weather. This may be true of some coasts, but it certainly is not the case with those of England, as the following facts show :—

On the Yorkshire coast, in the years 1868-69, fog occurred fifty-one times, with a total duration of 254¾ hours. During the period of these fogs, the average direction of the wind was S. by E., and its force 4·28. The average force of the winds[1] blowing from the eastward of the meridian was 4·55, against an average of only 2·59 for those blowing from the westward. The strongest wind, with a force of 8, was from E.N.E. ; the next strongest, with a force of 7, from S.S.E. and S. ; and, of those of the highest strength, average 5·52, the average direction was S.E. None of the fogs occurred during calm.

In the same years, at the entrance of the Bristol Channel, fog occurred one hundred and twenty-five times, with a total duration of 713¾ hours. During fogs the average direction of the wind was S.W. by W., and its force 3·94 (including calms, of which there were only five instances). The average force of the winds blowing from the westward of the meridian was 4·15, against an average of 1·82 for those blowing from the eastward, of which there were only eleven instances. The strongest winds, which on ten occasions blew with a force of 8, had an average direction from W.S.W. ; the next strongest winds, which on seven occasions blew with a force of 7, had an average direction from S.W. by W. Those of the maximum strength, average 6, the average direction of which was W.S.W., blew on fifty-three occasions.

In the same years, near Holyhead, fog occurred one hundred and seventeen times, with a total duration of 698½ hours. During fogs the average direction of the wind was S.W. by S., and its force (including calms, of which there were only five instances) 4·79. The average force of the winds blowing from the westward of the meridian was 5·24, against an average of 3·65 for those blowing from the

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que les brumes ne se produisent pas, en général, dans ces circonstances atmosphériques ; elles arrivent presque toujours du large, il est vrai, mais conduites par une brise légère."—Reynaud, "Mémoire sur l'éclairage et le balisage des côtes de France," pp. 273, 274.

[1] The force of the wind is stated in this Paper according to what is known as the 'Beaufort scale,' adopted by the Meteorological Department and the Admiralty, ranging from 0 = calm to 12 = hurricane. Colonel Sir H. James, R.E., at page 31 of "Instructions for taking Meteorological Observations," gives a table of pressures per square inch which he considers equivalent to the numbers on the Beaufort scale ; but the subject is one upon which authorities differ.

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eastward, of which there were but seventeen instances. The strongest winds, which on three occasions blew with a force of 9, had an average direction from S.W.; the next strongest winds on five occasions blew with a force of 8 from W.S.W., and once from N.E. Those of the maximum strength, average 6·22, the average direction of which was S.W., blew on sixty-seven occasions.

These figures indicate a prevalence and duration of foggy weather which must cover the date of arrival of a large amount of shipping on our coasts—a lee shore; the strongest winds blowing from seaward, and varying in force from a mean of 4·55 and a maximum of 8 on the eastern coast, to a mean of 4·47 and a maximum of 9 on the western coast.

Even in situations where fogs may not usually be accompanied by high winds, the necessity of power and range in fog-signals is in no way diminished; for the experienced seaman is aware that the heavy snow-storm, or the thick driving sleet and rain, which often accompany a gale of wind, are not less blinding and bewildering to him, when endeavouring to make a landfall or to discern a light, than the densest fog. The wreck of the 'Dunbar' at Port Jackson Heads on the night of the 20th of August, 1857, took place during a heavy gale, when the atmosphere was so thick that the ship ran on shore at the foot of the cliffs from which a revolving light of the second order is exhibited; and the sole survivor of the catastrophe emphatically stated that the light was never once seen. A more recent instance occurred in the case of the steamship 'Cambria,' which was lost on the night of the 19th of October, 1870, at Inistrahull, on the north coast of Ireland, in a heavy gale. There is, at the scene of this wreck, a revolving catoptric light of the first order, and it is affirmed that this light was never sighted. To which it may be further added, that, in heavy weather, a point of danger is most to be dreaded when it lies to leeward, and that, in such a case, the sound of the fog-signal is borne away from those for whose ears it is intended.

In 1854 some experiments, on different means of producing sounds for the purposes of fog-signals, were made by the engineers of the French Lighthouse Department.

In 1861-62 MM. Le Gros and Saint Ange Allard, of the Corps des Ponts et Chaussées, conducted a series of experiments upon the sound of bells, and upon various methods of striking them.

In November 1863, and January 1864, a committee of the Elder Brethren of the Trinity House of London made some experiments at Dungeness upon various fog-signals.

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The above are the only published records of experiments, specially upon fog-signals for coast purposes, which the Author has been able to find. The results obtained, and the conclusions arrived at, together with some special trials of particular instruments, will be referred to subsequently.

It is stated[1] that, in 1864, a commission appointed by the Lighthouse Board of the United States instituted a series of experiments to determine the relative power of various fog-signals which were brought to the notice of the Board. The experiments were conducted by Professor Henry of the Smithsonian Institution, Washington; but the Author has not succeeded in finding any recorded account of them.

In June, 1863, a committee of the British Association for the Advancement of Science, consisting of the Rev. Dr. Robinson, Hon. M. Inst. C.E., Dr. Gladstone, and Professors Wheatstone and Hennessy, memorialized the President of the Board of Trade, with the view of inducing him to institute a series of experiments upon fog-signals. The memorial, after briefly setting forth a statement of the nature and importance of the subject, described what was then known respecting it, and several suggestions were made as to the nature of the experiments proposed. It was pointed out that, in the first place, it is necessary to obtain some definite standard of measure of the power of fogs to arrest sound; and it was contended that this should be the first point for investigation, since otherwise any results obtained from phonic signals would be full of anomalies. It was observed that the minimum of efficiency of a fog-signal "is when a ship, putting down her helm on hearing the signal, can just come round clear of the danger. For very large steamers this cannot be done in less than 2 miles; and, allowing for sea and currents, that limit should at least be doubled; so that it may be assumed as a law, that (except in harbour) all fog-signals should be distinctly audible for at least 4 miles under every circumstance."[2] The memorial then alluded to the great difference which is known to exist, as respects power of penetration and range, between the volume and the intensity of sound, and to the necessity of instituting a careful examination of that branch of the subject. Reference was made to the experiments of M. Colladon and others upon the transmission of sound through water; and, lastly, a detailed list was given of the instruments upon which it appeared

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[1] Mr. F. S. Daboll to the Author, January, 1871.

[2] *Vide* "Report of the British Association," vol. xxxii., p. 105 *et seq.*

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most desirable to experiment, and a general programme of the experiments was set forth.

The proposal does not appear to have been favourably entertained by the authorities to whom it was referred, and the experiments were not undertaken.

The general tendency of fog to intercept or modify sound in its passage through the air, and the absence of reliable data as to the precise conditions which affect or vary the intensity of its action in this respect, have led to the consideration whether water may not be employed as a medium for the transmission of fog-signals. In 1851 Mr. Charles Babbage communicated to the United States Lighthouse Board a memorandum of various suggestions for the improvement of lights and beacons; and, in the course of his remarks,[1] he recommended that experiments should be made upon the transmission of sound through water. Professor Hennessy also has strongly advocated the investigation of the subject. He states,[2] that almost all the knowledge possessed respecting it is due to M. Colladon, whose experiments were made on the Lake of Geneva in 1826. M. Colladon found that subaqueous sounds were totally reflected at the surface, at such angles as rendered it impossible to hear them above water for distances exceeding about 220 yards. By the use of an instrument contrived for listening below water, M. Colladon was able to hear the sound of a bell under the surface at about 8¼ miles; and he subsequently succeeded in transmitting distinctly audible sounds under water to a distance of about 21¾ miles. He also found that the noise of the winds and waves produced little or no effect in diminishing the subaqueous sound.

It would be foreign to the scope of the present Paper to enter more into detail upon this branch of the subject; but it appears to open a promising field of inquiry, and it is in the highest degree probable, that research and experiment in this direction will ultimately lead to important modifications in the principle of fog-signals.[3]

In the details of experiments hereinafter mentioned, the condi-

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[1] *Vide* "Report of United States Lighthouse Board," 1852, p. 163.

[2] *Vide* "Report of the British Association," 1861, vol. xxx., p. 173.

[3] Those who desire to refer to the original experiments will find them recorded in "Mémoires présentés par divers savants à l'Académie Royale des Sciences de l'Institut de France," tom. v., p. 320; and in "Annales de Chimie et de Physique," 3me série, tom. ii., p. 525. An account of some later experiments on the same subject, by the Rev. Dr. Robinson, Dr. Gladstone, and Professors Hennessy and Wheatstone, will be found in the "Report of the British Association," vol. xxxv., p. 212.

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tions of wind and weather are carefully noted, so far as they can be ascertained. It need scarcely be added that these conditions materially affect the value of deductions drawn from the experiments themselves. Fogs have a remarkable power of deadening sound, and act in this respect so irregularly, that experiments made during clear weather have little or no practical value, except as mere competitive trials of different instruments; and, until the knowledge respecting the laws which govern the transmission of various sounds through fog is considerably extended, it cannot even be safely assumed that the instrument which has the greatest range and distinctness in clear weather will invariably maintain its superiority under altered atmospheric conditions. It is only by a series of experiments, systematically conducted, and carried out with persevering and rigorous accuracy under a great variety of circumstances as to wind and weather, and especially during fogs and equivalent atmospheric obstructions, that any really valuable addition can be made to the stock of knowledge upon the subject: without this, all investigation of isolated cases can be little better than a vague groping in the dark; and it is to be hoped that a thorough and scientific inquiry may yet be undertaken by competent authority.

The instruments in use for fog-signals are gongs, bells, guns, whistles, and trumpets; the two latter sounded either by steam or by condensed air.

Gongs.

The gongs employed are mostly of Chinese manufacture. They vary in size from about 1 foot 7 inches to 2 feet in diameter, and 4 inches to 4¾ inches in depth, and weigh from about 14 lbs. to 35 lbs. or 40 lbs. Their pitch and tone vary, but the most usual note is C, D, or F, all more or less imperfect. They are suspended by a loop of stout line, and are struck by hand with an elastic-headed gong-stick. The Author is not aware that any of those in use are sounded by machinery. There appears to be no particular duration or interval prescribed for their sounding during fog. They emit a peculiar and characteristic sound, which is well adapted to catch the ear, but their power is small. Great diversity of opinion exists respecting the range of their sound. Reynaud states that it scarcely exceeds 500 mètres (550 yards), but omits to mention whether this is a maximum or an average. As a maximum, it is much understated. The gong of the 'Warner' light-vessel has been heard at the Nab, a distance of 3 miles, under presumably

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favourable circumstances. There are, however, numerous instances in which gongs could not be heard at a distance of ¼ mile; and it is almost certain that their effect against a strong wind can be but slight. In the absence of accurate experiments, it is impossible to determine whether an increase in size would be accompanied with an increase of sonorous power sufficiently great in proportion to render such an increased size advisable. These instruments have the great recommendation of diffusing their sound equally all round, with but slight reference to the direction in which they are turned or struck, and they are therefore more suitable for use on board light-vessels (the situation in which they are chiefly employed) than for stations on shore.

Bells.

The bells used for fog-signals are of various weights, ranging from 3 cwt. to 2 tons and upwards. Those at rock lighthouses on the English coast weigh usually from 5 cwt. to 7 cwt.

The result of the experiments already mentioned, made by French engineers, may be summarized as follows:—

It was found that a better sound was produced by striking with a hammer than by the action of a tongue set in motion by swinging the bell. The range of the sound increases with the rapidity of the strokes, the range of sound produced by twenty-five strokes per minute being to that of fifteen strokes per minute as 1·14 to 1, and the range of sixty strokes per minute being to that of fifteen strokes per minute as 1·29 to 1. A bell weighing 220 lbs., struck by a hammer weighing 11 lbs. falling 7⅞ inches and immediately set off by a spring, was heard at about 1,320 yards against a fresh breeze (*bonne brise*); at 1¼ mile with the wind across; and at 2½ miles down the wind. In a calm, or with a light breeze across the line of observation, the sound was heard at 1½ mile. A hemispherical iron reflector, 4 feet 11 inches in diameter, encased in Portland cement, increased the mean range in the ratio of 147 : 100, over an arc of 60°, but from thence the increase due to the reflector diminished over an arc of 100°, beyond which an isolated bell had the advantage. In calm, or down the wind, the ratio of range over the same arc was as 170 : 100. Against a fresh breeze the ratio was as 130 : 100. It was found that a breeze across the axis of the reflector deflected the line of maximum range to nearly the same extent as that of the isolated bell; but it does not appear to have been ascertained whether, under these circumstances, the angular measurement of the arc of maximum range underwent any alteration. The same bell with reflector,

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struck by a hammer having the same weight and fall as before, was then tested against an isolated bell weighing 495 lbs., struck by a hammer having the same fall, but weighing 19 lbs., and each bell was struck sixty times per minute. With a fresh breeze down the line of observation, the range of the isolated bell was 6,754 yards or 3·80 miles, and that of the bell with reflector 8,387 yards or 4·75 miles; and, over an arc of 60°, the average range of the heavy isolated bell was to that of the lighter bell with reflector as 100 : 128.

These trials were not extended to foggy weather.

The experiments made in 1863 and 1864 by a committee of the Elder Brethren of the Trinity House, of which a Report was published by order of Parliament,[1] gave, with reference to bells, results differing considerably from those above described. The bell was an experimental one of steel, weighing 10 cwt., and was struck thirty times per minute, but no details are given in the Report respecting the weight and fall of the hammer. In the experiments of November, 1863, it was found that, at 1 mile distance, with a moderately fresh breeze across, the sound of the bell could not be heard so as to be identified as a signal. In those of January, 1864, in a calm with light variable flaws, the bell was heard well at 1 mile, faintly at 2 miles, very faintly at 2½ miles, and not at all at 3 miles and upwards. At 1 mile, with the paddles of the committee's steamer going, the bell was not distinctly heard. The maximum range of a 10-cwt. bell, therefore, with a fresh breeze across, was found to be less than 1 mile; whereas the French experiments gave 1¼ mile as the range of a 220-lb. bell under the same circumstances. It is, however, to be borne in mind that steel has considerably less sonorous power than bell-metal, and the discrepancy is no doubt partly owing to the difference in the material of the bells employed.

These trials were not made during foggy weather.

The bell at Howth, on the Irish coast, weighing 2¼ tons, and struck four times per minute by a hammer weighing 60 lbs. falling 10 inches, has been heard only 1 mile to windward in fog against a light breeze. A similar bell at Kingstown, struck eight times per minute, has "been found useful" by the Holyhead packets in fog at 3 miles,[2] wind not stated. In connection with this bell, it

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[1] Ordered by the House of Commons to be printed, March 14th, 1864. Parliamentary Paper No. 125.

[2] Secretary of the Irish Ballast Board to the Secretary of the Northern Lights Commissioners, December, 1862.

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should be noted that the Holyhead packets are powerful steamers, with very large paddle-wheels, so that it may be reasonably inferred that the sound of the bell is good, if found useful to a vessel of that description under way at the distance stated.

Capt. Walden, of the United States Revenue service, states that a bell at the Outer Light at Boston, Mass., weighing 1,375 lbs., and struck by machinery every 47 seconds, was distinctly heard by him on board a small steamer under full way, at a distance of 5 miles at sea, with the wind across and ahead;[1] whether during fog or otherwise is not mentioned.

It is asserted[2] that a bell at Cape Elizabeth, near Portland, Maine, weighing 1,560 lbs., and struck by machinery every ten seconds, was heard only 2 miles or 3 miles in calm weather, and was not audible for more than 100 yards against a light breeze.

Bells used for fog-signals at rock lighthouses are suspended by brackets or cantilevers from the pedestal of the lantern or from the gallery, and are struck by a hammer set in motion by clockwork driven by a weight. Owing to the exposed situation of these lighthouses, the bells are not always safe from injury, notwithstanding the height above the sea-level—from 60 feet to 100 feet—at which they are suspended. During a heavy storm on the night of the 30th of January, 1860, the bell at the Bishop Rock lighthouse, weighing 3 cwt., and suspended at a height of 100 feet above high-water mark, was torn from its fastenings and broken by the violence of the sea.

As a precaution against accident, therefore, the bell is always sought to be placed at that side of the tower which is least exposed; a position in which, though its safety may be secured as far as possible, its usefulness as a signal is reduced to a minimum, since the tower intercepts and tends to deaden its sound over an arc of not less than 90°, in the direction in which it is required to have the longest range for the benefit of vessels arriving from seaward during thick weather, and to those on board of which the sound of the bell may be the only indication of their whereabouts. It is, unfortunately, more easy to criticize the defects of present arrangements than to devise schemes of improvement. To place the bell on the most exposed side, would be to incur the risk of accident, and the consequent destruction of what small service it may render. To suspend it by a framework above and attached to the roof, would add to the weight, already sufficiently great, which

[1] *Vide* "Report of United States Lighthouse Board," 1852, p. 473.
[2] *Ibid*, p. 190.

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the lantern framing has to support; and if the standards of the superimposed framework were brought down and secured to the stonework of the tower itself, their great length would render them liable to too much vibration in storms. It is conceivable that a bell of hemispherical shape, similar to that of a clock or to that used as a guard's signal on locomotive engines, might be attached to the cone supporting the ventilating ball, and might even be made to form a protection to the cone, and thus allow the latter to be so reduced in weight as in a measure to compensate for the addition.

The patents connected with fog-bells are numerous. Most of them relate to mere details of the mechanism for striking. The earliest which the Author has met with, is that of Mr. D. Johns, who, in 1849, patented in the United States a method of ringing, and an adjustable clapper, to produce varied signals to indicate the locality. That of Mr. C. F. Stansbury, 1855, was for a mode of ringing by a lever carrying at one end the armature of an electro-magnet. In this scheme, the electric circuit was alternately completed and broken by wires attached to the verge of the escapement of a clock, which were dipped into cups of mercury as the pendulum oscillated; the apparatus being therefore adapted for the isochronous sounding of a bell at a distance from shore. Mr. C. Oliver's patent, 1861, referred chiefly to modifications of the clock-work for ringing; but it included a novel and ingenious device for ringing bell-buoys, by means of heavy spheres placed in troughs set end-on to the bell, which spheres rolled to and from the bell as the buoy moved with the sea. All such matters, however ingenious some of them may be, do not affect the question of sound and power of the bell as a fog-signal, and must be passed over with the above brief notice.

Guns.

Cannon, and explosive signals generally, were excluded from the experiments made by the French engineers in 1854, the reason assigned being, that their use is attended with inconvenience and even danger.[1] At an isolated rock light, such a signal could not be used; and it is not unreasonable to suppose that, in the close vicinity of any lighthouse, the discharge of a heavy gun would be injurious. The experiment, however, is about to be tried on a large scale at Flatholm, by the establishment of a battery of heavy guns for defensive purposes within 80 feet of the lighthouse

[1] *Vide* Reynaud, *ubi sup.*, p. 272.

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tower, and the result will probably settle the question in a practical manner. . . . .

Apart from considerations of safety and convenience, guns must be admitted to be, in many cases, efficient as fog-signals, and they are in use at several points on the English coasts. Those at Flamboro' Head, Lundy Island, and North Stack, are 18-pounders, and, during fog or other atmospheric obstruction, are fired every quarter of an hour, with a charge of 3 lbs. of powder. The light-vessels on the English coast are provided each with two 12-pounder carronades, firing a charge of 1 lb. or 1¼ lb. of powder; but these are not, strictly speaking, used as fog-signals, but rather as warning signals to a vessel seen standing into danger, and as alarms in case of wreck.

The Lundy Island gun has frequently been heard by the Author at Hartland Point, a distance of 10 miles, during dense fog, with a light breeze across the sound. It gave a dull faint report; and a moderate noise in his immediate neighbourhood would probably have been sufficient to drown it, unless the signal had been attentively listened for.

The gun of the Kish light-vessel is stated by the secretary of the Irish Ballast Board to be favourably reported on by the masters of the Holyhead steamers, and has been heard at a distance of upwards of 2½ miles.[1] But details as to direction and force of wind are wanting, to serve for a judgment of the absolute efficiency of the signal in these cases. During a dense fog at Kingstown, in October, 1859, guns were fired from the guard-ship, for the guidance of one of the Holyhead packets, and the report of some of them was not heard at 1 mile distance.[2]

In the Trinity House experiments of January, 1864, made in calms and light airs, but not during fog or thick weather, an 18-pounder gun with a charge of 3 lbs. of powder was tried, with the following results. At distances of 2 miles and 3 miles, with the paddles going, the flash and smoke were seen, but no report was heard. When the paddles were kept just moving, the report was pretty good at 2 miles. With the paddles stopped, the report at 2 miles and 3 miles was very good, and at 4 miles it was faint.

It is a noteworthy circumstance, that the height at which a gun is situated above the water appears to be no detriment to its value as a signal. Such at least is the inference from the following case,

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[1] Letter to the Secretary of the Northern Lights Commissioners, December, 1862.

[2] *Vide* Fitzroy, "Weather Book," p. 315.

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mentioned by Dr. Gladstone: "Captains Close and Nisbet, of the Trinity Board, have made some experiments . . . . at Holyhead mountain, where a gun was fired from near the surface of the sea, and another at a considerable height, and the respective reports were listened to from various distances at sea. They found the upper gun was heard best for six miles, after which it lost its superiority. But the most remarkable result was the irregularity of the noise from the lower gun, which at certain points, in fact, was not heard at all, though the flash was distinctly seen."[1]

The cost of a fog-gun establishment may be taken at about £105 for two 18-pounder guns, &c., complete, and £850 for buildings, exclusive of purchase of land; together £955. The annual expenditure, reckoned upon the average at three stations during the year 1869, is as follows:—

	£	s.	d.
Wages of two gunners	84	0	0
Clothes and allowances	25	4	0
Stores, ammunition, &c. . . .	118	16	8
Total . . .	£228	0	8

or at the rate of £18 1*s.* 10*d.* per twenty-four hours of fog as averaged at the same stations during the same period. Exclusive of wages and allowances, it is at the rate of £9 8*s.* 1*d.*, per twenty-four hours of fog.

Whistles.

The sharp and piercing scream of the hemispherical or locomotive whistle possesses, in a very high degree, the quality of intensity as compared with volume, which is considered essential to a thoroughly efficient fog-signal. It is difficult to trace the first application of this instrument to the purposes of coast signals; the earlier attempts having been made rather with the view of adapting it as a warning signal on board vessels at sea. The late Mr. Alexander Gordon, M. Inst. C.E., appears to have been the first to suggest its use in combination with a reflector, for the purpose of collecting and condensing its sound into a phonic beam.[2] He proposed to sound the whistle by either air or steam; and to place it either in the double reflector of Bordier Marcet, for sounding equally all round the horizon, or in a focus common to three parabolic reflectors grouped together,

[1] *Vide* "Proceedings of the Royal Institution," vol. iv., p. 54.

[2] *Vide* "Evidence before Select Committee on Lighthouses," 1845, Nos. 4,453–4,457.

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revolving on a vertical axis. In the course of his evidence he pointed out that a shrill sound, high in the scale, is carried much further than a lower note, although the sound of the latter may be more powerful when near at hand. "Shrill sounds," he says, "penetrate further, and are better heard." But he stated that he did not know of any instance of the locomotive whistle having been applied to the purpose of a fog-signal.

In 1850 the late Mr. C. L. Daboll brought out a whistle, sounded by compressed air. One of these machines was erected by order of the United States Government, at Beaver-tail Point, near Newport, R.I., in 1851. It had a condenser of 225 gallons capacity, filled by two air-pumps 3 inches in diameter, and 8 inches stroke, worked from a cranked axle driven by simple gearing, and moved by one horse. The condenser could be filled to a pressure of from 40 lbs. to 50 lbs., in 7½ minutes by slow gearing, and in 4 minutes by fast gearing; and the whistle sounded once every 3 minutes. It was estimated by Captain Walden, who made an official examination of the apparatus, that, in order to maintain a regular series of signals, it would require the work of one horse during one-third of the time over which the signals extended [or at the rate of one-third of a horse-power per hour].

The testimony as to the efficiency of this whistle is abundant and favourable. During dense fogs (wind not stated), it was distinctly heard at from 6 miles to 8 miles, in the town of Newport even when there was considerable noise of drays and carriages in the streets. It was distinctly heard in the same town at 6 miles in dense fog, against a light breeze, early in the morning, when there presumably was but little noise in the streets; and at Jamestown, distant 5 miles, in foggy weather, with the wind (strength not stated) blowing across the sound. Captain Walden heard it distinctly at a distance of 2½ miles, in a rough sea, and with a strong wind blowing across the sound.[1] The President of the United States Lighthouse Board bears testimony to its efficiency as a signal, from personal observation. He says: "The early part of the night was clear, but as the steamer approached the east end of Long Island and the passage between Beaver-tail and Brenton's reef, the fog became so dense that navigation would have been extremely hazardous without some better guide than the compass and lead; and this was found in the whistle, which sent its clear and shrill notes far over the water, indicating the bearing of the point on which it is placed, much more accurately than a bell or a

[1] *Vide* "Report of the United States Lighthouse Board," 1852, pp. 466–473.

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gun would have done, and enabling the steamer to arrive, with little loss of time, at her port."[1]

The estimated cost of four of these machines, three of them worked by horse, and one by hand-power, including buildings for two of them, was at that time $3,500,[2] or £729.

In 1854 Professor J. C. Cresson, an American authority, proposing the employment of a steam-whistle, made a calculation of the quantity of steam required to sound it; which, for a whistle intended to be heard 5 miles or 6 miles (under what conditions of wind is not stated), blowing blasts of 2 seconds duration, with intervals of from 2 minutes to 3 minutes, he estimated at less than one-tenth of a horse-power[3] [per hour?]. But he gives no data for this calculation.

In 1856 Mr. William Smith patented in England a compound whistle, consisting of four whistles combined, with their sounding edges straight on plan instead of circular, of which a detailed description and drawing is to be found in his specification, No. 56/2105. It does not appear that any experiments with this instrument have been made public, and therefore its value as a fog-signal cannot be determined.

In January, 1860, Mr. E. A. Cowper, M. Inst. C.E., communicated to the Trinity House of London a design for a fog-signal, in which it was proposed to employ a whistle 15 inches in diameter, placed in the focus of a parabolic sound-trumpet, 25 feet long, and blown by steam from a 12 H.P. boiler. This apparatus does not appear to have been tested in practice, but its large size leads to the expectation that it would be an instrument of great power. Mr. Cowper also, at that time, perceived and pointed out the propriety of making the intervals and character of the sounds of a fog-signal to correspond with the character of the light to which it is auxiliary.

In 1861 Mr. Delabere Barker patented in England (No. 61/1031) a fog-whistle to be sounded by superheated steam, being a communication from Mr. T. T. Vernon Smith, of St. John's, New Brunswick. The claim of the invention is: "1st. The employment of high-pressure or superheated steam for sounding a whistle as a telegraphic signal at sea, operated upon by machinery at certain intervals of time, so as to signal not only by the note, but by the

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[1] *Vide* "Report of the United States Lighthouse Board," 1852, p. 466.

[2] *Ibid.*, p. 467.

[3] *Vide* "On the Use of Phonic Signals in Navigation," Journal Franklin Inst., 3rd Series, vol. xxviii,, p. 397.

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interval between the notes. 2nd. The employment of two or more whistles, thus operated upon, to give an identity to the locality in which they are used, and to prevent the confusion with other signals. 3rd. The use of reflectors to concentrate and intensify the sound of steam-whistles, and tubes to conduct the same in different directions." The apparatus of this machine is extremely simple. The boiler is vertical and tubular, surmounted by a steam-chest or dome for superheating the steam. Connected with this is a whistle or whistles, from 5 inches to 6 inches in diameter, sounded by clockwork, or other suitable automatic movement, for opening the valve of the steam-pipe at regulated intervals.

An apparatus of this description, the boiler of which is capable of working about 2 H.P., has been for many years in operation at Partridge Island, St. John's harbour, New Brunswick. It appears to have been established at some time between 1850-60. Through the courtesy of the authorities at the Colonial Office and the Board of Trade, his obligations to whom it affords him much pleasure here to acknowledge, the Author is enabled to lay before the Institution some particulars relating to this apparatus.

An official communication from the Commissioner of Public Institutions,[1] after describing the general character of the machinery employed, states that the whistle is sounded once a minute, the blast lasting ten seconds. "The pressure of steam under which it is intended to be worked is about 100 lbs. on the inch, the safety valve blowing off at 105 lbs. With a pressure of from 90 lbs. to 95 lbs. the whistle can easily be heard in calm weather a distance of 6 or 7 miles, and has been heard by a steamer in a heavy gale, and sea running high, at a distance of 5½ miles to windward, but it was then not audible till the paddles were stopped.

"In August, 1863, Commander Malcolm, H.M.S. 'Barracouta,' made some experiments with regard to the steam-whistle, and reported to the Admiral on the station, of which the following is an extract.

"'On the 8th instant, in Her Majesty's ship under my command, when sailing from St. John in a dense fog, the horn bearing about N. by W. half a mile, wind S.W., force 4, I steered S.W. by S., put the patent log overboard, and when we lost the sound the patent log showed 2½ miles. So, on this occasion, we may infer, being to windward of horn about 3 miles, fog being very dense, and breeze moderate (about 4), that the sound of the horn, in such

[1] Letter to the Hon. S. L. Tilley, March 23, 1864.

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weather, may fairly be supposed to have a radius of from 3 to 6 miles.'

"Shipmasters and pilots frequenting this port have uniformly expressed their sense of its value.

"The Chamber of Commerce passed a resolution acknowledging the advantages derived from its use, and it is a well-established fact that many vessels have been enabled to make this port guided by this alarm, when without it they must have remained in the bay in the worst weather. . . . The result of the last two years' operations gives an average expense of 21*s.* 8½*d.* [19*s.* 10*d.* sterling] for twenty-four hours' working, this covering everything but wages, which . . . are probably here at a minimum."

The above report enclosed also a letter from the parties who erected the apparatus, making some suggestions for improvement in the details of its machinery, which would allow the engine to be reduced in size. They state: "As the power required to work the horn is very small, the boiler would be about 3 feet diameter, with 61 tubes 2 inches diameter and 6 feet long, capable of working 6 H.P., and calculated to sustain a working pressure of 200 lbs. to the square inch. The horn would be larger than that on Partridge Island, which . . . . would enable it to be heard at a much greater distance. The cost of one similar to the one now in use would be about £200 sterling. The cost of one of the improved . . . would be about £240 sterling."[1]

A later Report[2] states that the whistle on Partridge Island "was found to be most useful both to steamers and sailing vessels, at times when the light on the island was found to be perfectly useless, and vessels have frequently been enabled to make their way up the harbour to their wharf or anchorage-ground without even seeing either land or light, solely by the aid of the fog whistle. As the usefulness of this description of navigation security became better known, the American Government sent their officers to examine it, and now there are several in operation on the American coast.

"The British Admirals have also visited the one at St. John Harbour, and have highly approved of it.

"The Trinity House of London has also obtained plans of it from this department, and one has recently been erected at Aberdeen Harbour.

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[1] Messrs. Fleming and Humbert to Commander Mulcolm, R.N., 8th August, 1863.

[2] *Vide* "Report by the Minister of Marine and Fisheries," Ottawa, 2nd No., 1870.

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"The Government of Canada has also extended their use, and there are now four in operation, viz., one at Partridge Island, one at Lepreau, one at Yarmouth, and one at Seal Islands. There are also two more constructing; one for Cranberry Island, Cape Canso, and one for Anticosti; and arrangements are now being made for placing one in a new iron light-ship, which this department is building for Red Island reef, River St. Lawrence."

The cost of an engine, boiler, and whistle complete, is estimated in the above Report at $2500 (about £521 sterling), or more than double the cost estimated in 1863. It is not easy to reconcile this discrepancy, unless it be supposed that wages and prices have greatly risen, and that experience has suggested additional alterations and improvements, leading to increased cost.

With a view of further developing the capabilities of the whistle as a fog-signal, it is much to be desired that periodical reports upon the performances of these instruments, and upon alterations and improvements made from time to time in their construction, should be obtained from the authorities in the localities where they are in use. The bay of Fundy, and many other parts of the North American coast, are so frequently beset with dense fogs as to afford ample opportunity for examination of their effect upon phonic signals.

The whistle at Aberdeen to which reference has been made, was established by the Aberdeen Harbour Commissioners in December, 1869, from the designs of their engineer, Mr. W. D. Cay, Assoc. Inst. C.E., to whom the Author is indebted for the information respecting it. It is on the Vernon Smith principle; the whistle is 6 inches in diameter, and is sounded at intervals of one minute, the blasts lasting ten seconds. It is sounded by steam at a pressure of 60 lbs., supplied from a tubular boiler. Mr. Cay states[1] that he has no reports from the captains of vessels as to the efficiency of the signal, but has been given to understand that it is very useful to captains of steam vessels seeking the harbour in foggy weather. He has also experimented on it at sea in clear weather in a tug, and heard it quite distinctly 5 miles with the wind and 2 miles against the wind. In foggy weather it can be distinctly heard at about 2½ miles, with a hill and houses intervening; and it is said to be a great annoyance to the inhabitants of Aberdeen living 2 miles to 3 miles from it.

The cost of the building and apparatus for this signal was £405 16*s.* 1*d.*; and its working expenses in 1870 were as follows:—

[1] Letter to the Author, January 27th, 1871.

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	£	s.	d.
Wages of keepers	49	8	5
Coke	13	5	6
Sundries	4	8	0
Total . .	£67	1	11

or at the rate of £4 5*s.* 1*d.* per twenty-four hours of working. Exclusive of wages, the working expense per twenty-four hours was 22*s.* 5*d.* The amount expended for wages is moderate, the keepers being employed on the other harbour works when the signal is not in action.[1]

In 1865 Mr. H. T. Humphreys, M. Inst. C.E., devised an apparatus for automatically sounding a whistle, in which the motive power is afforded by the rise and fall of the tide. The tide, while flowing, acts upon a turbine, and, while ebbing, upon a second turbine, of a power calculated for the rise and fall of the tide at the particular station where the signal is to be placed. These turbines, one of which is constantly at work except during the period of slack water, are contrived to drive a pump, which delivers the sea-water into an elevated tank. The water thus raised is employed to drive a smaller turbine, which, by means of a cam, raises and lets fall a heavy ram or hammer. This falls upon a piston working air-tight in a cylinder, the valves of which are so arranged as to admit air into the cylinder when the piston is raised, and to pass out the contained air, when compressed by the blow of the hammer, into a whistle or whistles. The cam revolving, lets the hammer fall upon the piston, suddenly compressing the air in the cylinder and driving it with violence through the whistle, producing a note of great shrillness and intensity—hence the name "Clamor Æstus," or "Tide-shriek," applied to the apparatus. As soon as the cam lifts the hammer, a spiral spring returns the piston to its place in the upper end of the cylinder, ready for another blow. The amount of water pumped into the tank is so regulated as to provide a supply for efficiently working the turbine which lifts the hammer, during the periodical inaction of the tidal turbines at slack water. It was intended to use phonic reflectors, or a trumpet-shaped blast-director, in combination with the whistle, if required for the purpose of condensing the sound in particular cases. The inventor's calculation of the relative proportions of his apparatus, requisite to produce a 2-feet blow

[1] Mr. W. D. Cay to the Author, January 27th, 1871.

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from a 1-ton hammer twice a minute, gives ⅛ H.P. for each of the tidal turbines where the maximum range of tide is 6 feet.

The expense of the construction of a complete apparatus on the "Clamor Æstus" principle prevented the trial of its powers in actual practice. The experiments made by Mr. Humphreys had for their object the establishment of his idea of the advantages of a percussive blast of air through a whistle. He considers that those advantages were fully proved to exist; and that the peculiar and entirely characteristic sound produced by the fall of a heavy weight on an air-vessel would be superior to that produced by a fog-horn or steam-whistle, and could be at once distinguished from all other sounds, and its source recognized most readily.[1] Mr. Humphreys's invention deserves notice, not only on account of the novel and ingenious idea of a percussive blast, but also as a workmanlike attempt at solving the problem of applying tidal action to the purpose of a fog-signal.

Before quitting the subject of the whistle as a fog-signal, it should be added that there undoubtedly is room for improvement in the construction of the instrument itself. It appears, from an experiment made by Professor Holmes, that even in the best specimens of the hemispherical whistle the steam or air does not act upon the whole circumference of the sounding edge. To prove this, he interposed a metal plate between the lip and the slot at those parts where no sound was produced, and found that, in a 10-inch whistle of the best make by Flaud, of Paris, the blank portions amounted to fourteen-fifteenths of the whole; the sound of the whistle in this condition being in no degree diminished, but, if anything, improved, thus showing that the portions blanked had no effect in augmenting the sound, but that the non-effective steam rather interfered with the proper vibration of the metal. If, therefore, it be possible to construct a whistle, every portion of which shall be properly acted upon, it may be expected that an efficient instrument will be produced, probably twelve or fourteen times as powerful as the best specimen of similar size now obtainable. This, if ever it be accomplished —and it appears to be a mere problem in mechanical accuracy of manufacture—will be of the utmost importance with reference to the use of the whistle as a fog-signal, increasing in an extraordinary degree its intensity and range, without at all increasing its working expenses.

[1] Mr. H. T. Humphreys to the Author, January 24th, 1871.

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Trumpets.

The use of horns[1] as fog-signals is of old date; but, so long as the means of sounding them was limited to the human lungs, their power must have been too small to be effective at long distances. Apparently the earliest attempt to apply machinery to the purpose was to the 'Telephone,' which was laid before the Admiralty in 1844 by the late Admiral J. N. Tayler. The apparatus consisted of a condenser, filled by air-pumps worked from a cranked axle driven by a winch. A set of piston-keys allowed the air to pass into the sounding-pipes, which were four in number, having metallic springs or reeds to produce the sound. By combining and varying the notes, it was proposed to convey signals during fogs. It was stated that this instrument would convey a signal 6 miles or 8 miles in foggy weather; and, when a greater range was required, the inventor proposed to add to it a parabolic phonic reflector. It is believed that some experiments were tried with this apparatus, but the Author has not been able to discover any report upon them. The metallic reed of the pipe or trumpet may have furnished an idea to subsequent inventors.

In 1851 the late Mr. C. L. Daboll designed a trumpet, having a metallic reed, and sounded by compressed air, one of which was then in actual operation at Beaver-tail Point. It was attached to the condenser employed for the whistle, and appears to have been occasionally sounded in alternation with the latter, since both instruments are mentioned by some of the persons who testify to the range of the sound.[2] It is said to have been distinctly heard at 6 miles distance, against a light breeze and during dense fog; and Captain Walden states that he distinctly heard it at 2½ miles distance, in a rough sea, and with a strong wind across. Evidently the value of this instrument was fully recognized by the authorities, since Captain Walden proposed that similar ones should be established at certain places instead of whistles, as originally intended, in order to give the navigator a better opportunity of determining his position than if all the signals were of one character;[3] and it is in the highest degree

[1] The term 'horn' has been occasionally applied in the preceding section to the apparatus connected with the whistle. In the present section the term is restricted to those instruments which are of the nature of a bugle, and to those which are sounded with a reed.

[2] *Vide* "Report of the United States Lighthouse Board," 1852, pp. 471–472.

[3] *Ibid.*, p. 468.

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improbable that this recommendation would have been made unless he had been perfectly satisfied of the efficiency of the trumpet as a fog-signal.

In June, 1860, Mr. Daboll patented in the United States, and in November, 1863, in Great Britain (No. 63/2962), the application of air condensed by machinery to the sounding of whistles or horns, and the revolution of the horn whilst sounding. In this apparatus air is forced by suitable engine power into a condenser, and this by a pipe communicates with the trumpet or other wind instrument, a valve being interposed which is made to act by the engine employed to condense the air; and the length of the blasts and intervals of silence regulated by suitable gearing. The condensing engine and apparatus may be at a considerable distance from the trumpet, the compressed air being communicated by a pipe which becomes a continuation of the condenser, and the valve being worked from the distance by wires or rods. Where a method of sounding at a distance from the engine can be successfully adopted, it may prove of great importance, allowing the employment of the trumpet as a fog-signal in situations where, owing to the small space available, the exposed position of the site, or other local circumstances, the requisite buildings and machinery for the motive power could not be erected at the same spot. It will be seen below that the same end is attained by Professor Holmes in a different manner. The engine preferably used is Ericsson's caloric engine; which, owing to the short time required to set it to work, the low cost at which it can be worked, its simplicity and non-liability to get out of order, and its freedom from danger of explosion, appears to be very suitable for the purpose. It can be started to work in from twenty minutes to thirty minutes, a matter of no slight importance where it is requisite to commence sounding the signal promptly on the appearance of a fog; and, should the fog clear off, the fire can be immediately drawn, after which the engine will continue to work for twenty minutes or thirty minutes by its own heat, so that if the fog reappears there need be no delay in continuing the signal. And, as has been suggested by Mr. J. N. Douglass, M. Inst. C.E., if the fire-box of the engine be kept warm by a lamp burning inside, the time required to start the machine in the first instance may be reduced to less than ten minutes.

The rotation of the horn is a feature of great importance where only one is employed, as it permits the axis of the mouth to be directed to every point of the horizontal arc over which it is required to act, and thus to distribute the sound as it moves: the

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apparatus acts, in fact, as a revolving light; and, if the trumpet were continually sounding and revolving, the analogy would be perfect. But the analogy fails when the rotation is reciprocating, since the recurrence of the sound on the line of axis does not take place at an equal interval of time, but occurs sooner at the ends of the arc. The trumpet must either revolve entirely, or else be made to return in silence to the opposite end of the arc in time to commence sounding there again at the proper interval.

When the reciprocating rotation of the horn is to be uniform, it is accomplished by an eccentric-rod rocking a bevelled toothed quadrant, which drives a pinion keyed on the vertical shaft of the horn. When it is to be intermittent, it is effected in the following manner:—A mitred wheel, kept constantly revolving in one direction on a horizontal shaft, is geared into two mitred wheels, one above and one below, running loose on the shaft of the horn, and keeps these wheels in constant motion in opposite directions. Between the horizontal mitred wheels, and sliding on a feather on the shaft of the horn, is a clutch, which engages a corresponding clutch on either the upper or lower mitred wheel, according as it is lifted or depressed. A weighted arm on the axle of the clutch-lifter tends to keep the clutch constantly engaged with the upper wheel, and therefore to keep the horn always revolving in the same direction. Another arm from the axle of the clutch-lifter, bent downwards, and having a small friction-roller at its lower end, is moved by wipers on a horizontal wheel revolving with the horn. According to the shape of the wipers, the clutch is either retained in the middle between the horizontal mitred wheels, the horn being then stationary; or it is pressed down to engage the lower one, when the horn revolves in a direction the reverse of that produced by the upper one: where there are no wipers, the bent arm is not acted upon, and the weighted arm lifts the clutch, and engages it with the upper wheel. By making the wipers of suitable shape and number, the rotary motions of the horn, and its intervals of rest, can be regulated with the greatest nicety.

In the Trinity House experiments of November, 1863, before mentioned, the Daboll fog-trumpet employed was one of what is termed in America the third class, 4 feet 6 inches long, 2½ inches in diameter at the smaller end, and 21 inches at the mouth, and was worked by an 18-inch caloric engine.[1] It was a straight stationary trumpet, placed horizontally, 8 feet above the shingle of

[1] The denomination of the caloric engine is determined by the diameter of the outer piston.

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the beach, its mouth projecting about 2 feet 6 inches from the building containing the engine. The blasts continued four seconds, with intervals of twelve seconds. The weather was not foggy, and a moderately fresh wind was blowing across the line of axis of the horn. In a line with the axis, the horn was distinctly heard at 2½ miles, and the sound was observed to diminish as this line was departed from. When the line of observation was at right angles to the axis, the horn being then to leeward, it was not audible beyond ⅓ mile. In a similar position, but with the horn to windward, it was heard at a distance of 4 miles or 4½ miles.

In the experiments of January, 1864 the same apparatus was used, the pressure of air in the condenser being 10 lbs. The weather was calm, with variable flaws, and no fog. In the line of its axis, with the paddles going, the horn was not distinctly heard at 1 mile. With the paddles stopped, it was heard well at 2 miles, very fairly at 2½ miles, faintly but good for the distance (and, immediately afterwards, distinct and marked) at 3 miles, and not at all at 4 miles. At an angle of 45° from the axis of the horn, with the paddles stopped, it was distinctly heard at 1 mile, and faintly at 2 miles. At an angle of about 60° from the axis of the horn, with a light air across the line of observation, its sound was not good at 1 mile, and was faint at 2 miles.[1]

This apparatus was subsequently removed from Dungeness, and placed on board the Newarp light-vessel, where it is now used during fogs. At this station, it can be distinctly heard at 5½ miles distance, during foggy weather, against a light air, but not against a moderate breeze or during very dense fog. In clear weather it has been heard at about 4 miles, with a very light air across; and at about 7 miles down the same wind.

The Daboll apparatus which replaced it at Dungeness in 1865, and a similar one erected in 1868 at St. Catherine's in the Isle of Wight, are of the American second class. The trumpet is curved at the upper end, its main length being placed in a vertical position. Its axial length is 9 feet, with a diameter of 2 feet across the mouth and of 2¾ inches at the small end. The condensers are two in number; each 3 feet 3 inches in internal diameter, and 4 feet 4 inches in internal length. The apparatus is worked by a 24-inch caloric-engine, estimated to be of about 2½ H.P., and consuming 12 lbs. of coke per hour. The blasts are of five seconds duration, with intervals of twenty seconds at Dungeness and of fifteen seconds at St. Catherine's. At the commencement of

[1] *Vide* "Report of the Trinity Board Corporation," *ubi sup.*

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the blast the pressure in the condenser is 20 lbs., diminishing at its termination to 5 lbs., per square inch. The cost of the present engine and apparatus at Dungeness was about £1,238, and of that at St. Catherine's £1,338, both inclusive of expense of erection. The estimate for the requisite buildings will be given hereafter.

In 1863 Professor Holmes devised a trumpet, to be sounded by compressed air, having a reed of german silver, which, for high-pressure trumpets, is 5½ inches long, 1 inch wide, and $\frac{5}{16}$ inch thick; and for low-pressure, 6½ inches long, 2¾ inches wide, and $\frac{5}{16}$ inch thick. The leading feature of this trumpet, so far as its power is concerned, is, that it is constructed to vibrate in unison with its reed, and thus to form a truly homogeneous instrument; herein differing essentially from Daboll's, which acts merely as a condenser and conductor of the sound produced by the reed. The result is a considerable increase in the effective power of the signal. A report made in 1865 by a Commission appointed by the French Minister of Public Works, and presided over by M. Reynaud, states that, in clear weather, against a wind estimated at force 3, a steel bell weighing 8 cwt. was heard at 984 yards, a 10-inch whistle blown by steam at 75 lbs. per square inch was heard at 3,281 yards (1·88 mile), and Holmes' horn blown by air at 30 lbs. per square inch was heard at 7,655 yards (4·35 miles). To show the small power required to work the trumpet, the apparatus was subsequently disconnected from the engine, and was driven for two hours by a small horse of the same kind as the usual Parisian cab-horse.

Some experiments were subsequently made by the French Lighthouse Commissioners; in which it was found that Holmes' trumpet was heard at a distance of 7,108 yards (4 miles), against a slight breeze; whilst a steel bell weighing 2½ cwt. was not distinctly audible beyond about 2,187 yards (1¼ mile). And it is added, that the fishermen of the island of Moléne declare that a trumpet of the same description was heard there, in calm weather, when being experimented upon at Ushant, a distance of nearly 9½ miles.[1]

Another important improvement of Professor Holmes' is in the construction of the horn itself, which he makes of such a form, both in the conical body and in the bell part, that the issuing sound, instead of passing out only in the line of axis and immediately dying off on all sides, is perfectly uniform in intensity

[1] *Vide* "Exposition Universelle, 1867. Notices . . . réunis par les soins du Ministre . . . des Travaux publics," p. 333.

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throughout an angle of 90°; a problem requiring for its solution a thorough knowledge of the laws of sound, and an extreme nicety of calculation. Two light-vessels, recently constructed, from the designs of Mr. J. N. Douglass for the China Lights service, are each fitted with four of these trumpets, placed with the axes of their mouths at right angles to each other, forming a holophone round the whole horizon.

An apparatus embodying all Professor Holmes' latest improvements has recently been erected at the new lighthouse station at Souter Point. The condenser and trumpets are situated at a distance of about 350 feet from the air-pump, which is driven by the steam-engine employed for the generation of the magneto-electric current for the light, the air being conveyed to the condenser by a 2-inch cast-iron pipe. The trumpets, two in number, will be placed with the axes of their mouths at right angles to each other, so as to emit their sound over an arc of 180°, corresponding with the illuminated arc of the light. They will be sounded simultaneously at intervals of thirty seconds, by the following contrivance, which is another of Holmes' improvements. The only communication between the condenser on which the trumpets are fixed, and the engine-room, is the pipe which conducts the air; and, as soon as the pressure in the condenser rises to 30 lbs. per square inch, a tumbler is automatically thrown over, and the valve admitting the air to the trumpets is suddenly opened. When the pressure is reduced by the blast to 28 lbs., the valve is as suddenly closed by the tumbler falling over in the opposite direction, and the valve remains closed until the pressure again rises to 30 lbs. The intervals between the blasts are regulated by simply changing the capacity of the condenser, by pouring in more or less water, so that the air-pump shall require less or more time to recover the 2 lbs. of pressure expended. This ingenious contrivance obviates the necessity of using rods or wires for working the trumpet at a distance as proposed by Daboll; but it is a question of some interest, whether the air in the condenser may not become so charged with moisture from the water employed, as to be prejudicially affected by severe frost.

In order to utilize the whole of the sound, a considerable portion of which would be sent upwards at too great an angle, owing to the large phonic arc of the improved trumpet, Professor Holmes proposes to employ a kind of sounding-board, to be placed above the mouth, to send out the rising sounds in a horizontal line.

The patent of the Ericsson caloric engine having expired, it can now be manufactured free of royalty; and the cost of a 24-inch

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one, with one of the improved Holmes trumpets, is estimated at £450, and £20 for each additional trumpet.

The cost of an engine-house, keeper's dwelling, &c., for a fog-horn establishment, may be estimated at £1,500; this estimate being based upon the data afforded by experience in the work at St. Catherine's, which is selected on account of the permanent and substantial character of the buildings there erected. The cost of the engine and apparatus has to be added, according to the particular kind employed.

The working expenses of the establishment at St. Catherine's during the year 1869 were as follows:—

	£	s.	d.	£	s.	d.
Wages of one attendant . . .	49	10	0			
Clothing and allowances . . .	12	10	0			
				62	0	0
Stores	6	3	1			
Fuel, viz.:—Coke (two tons) . .	3	0	0			
Coal (one ton) . . .	1	8	0			
				10	11	1
Total . . .				£72	11	1

In the same year the fog-signal was worked during 279 hours 50 minutes, at a cost of £6 4*s.* 5*d.* per twenty-four hours. Exclusive of wages and allowances, the working cost per twenty-four hours was 18*s.* 1*d.*

Upon a review of the various fog-signals which have been mentioned, it will be at once perceived that the whistle and the trumpet stand out prominently as regards power and manageableness. Guns, besides their heavy working expenses, have the disadvantage of requiring a longer interval between the signals, and of entailing continuous work upon the attendant. It appears, therefore, that it is to the improvement and augmentation of power of the two former that a more efficient instrument must at present be chiefly looked for. Whatever may be the fog-signal adopted in practice, power of sound and certainty of action are indispensable conditions. Better, it has been said, no signal at all, than one that cannot be relied upon; and, undoubtedly, if the mariner is led to expect a signal at a certain place, and at a sufficient range to ensure him time to act upon its warning, it ought to be so heard with unfailing certainty. Among existing signals there are some which in ordinary fog and moderate weather will fulfil these requirements. But it is doubtful if they will act with certainty to windward against a heavy gale. The howling of the wind; the groaning

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and creaking of hull and spars; the shock and roar and thunder of the sea; the drenching, blinding spray, driven like a concentated battery of hail; the fierce blast, cutting like a knife, and striving as it were to stifle and beat back even the attempt to breathe; the thick mist, closing everywhere around, and shutting in the ship as if to be the solitary mark for all the wrath and savagery of the elements;—these are the antagonists against which the fog-signal has to try its powers; and powerful indeed must be its voice if it afford in time a friendly warning.

There is another point with respect to fog-signals where in connection with a lighthouse, which ought not to be overlooked; namely, the importance of making the character of the sounds, and their duration and intervals, correspond with the character of the light. The fixed light may be denoted by continuous sound, or better by a quick succession of sounds; the revolving and the flashing light by corresponding blasts; and coloured beams alternately with white light, by a lower and higher note alternating in a similar manner.

One difficulty in the way of employing, at rock lighthouses, any fog-signal but a bell, or such other instrument as can be sounded by the application of simple clockwork, is the unsuitableness of such buildings for the reception and working of a steam or caloric engine, and the severe labour which would be entailed upon the keepers by the use of powerful machinery worked by hand. But the Author still entertains the opinion which he formed sixteen years ago, that the vast dynamical power afforded by the rise and fall of the tide will yet be utilized and applied to the compression of air for the purposes of fog-signals at such stations.

In conclusion, the Author ventures to lay this Paper before the Institution; encouraged by the hope that it may be the means of directing more general attention to a subject, of which—intimately connected as it is with the peaceful interests of navigation and commerce, and with the safety of valuable property and priceless human life—it would be difficult to exaggerate the importance.

[Mr. A. Beazeley

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Mr. A. Beazeley said that he had received some information from Penzance which confirmed the views he had expressed as to the character of the wind during fogs. At Penzance, in the year 1870, fog occurred sixty-three times, with a total duration of 282 hours. During the period of these fogs the average force of the wind (including calms, of which there were only five instances) was 4·48, and the maximum 9. The wind on thirty-four occasions exceeded the average force, and amounted to 6·29.

In 1866 a third-class Daboll's fog-trumpet was established at Cumbrae, in the Frith of Clyde, by the Cumbrae Lighthouse Commissioners. He was informed by Mr. Graham, the superintendent, that it had been heard, during fog, at a distance of from 7 miles to 9 miles against light and moderate breeze, from 12 miles to 14 miles down the wind, and from 3 miles to 5 miles to windward against a strong breeze, and that it was highly approved by masters of vessels frequenting the Clyde, having frequently enabled them to steer for their destination during dense fog. The cost of the engine and apparatus was £600, and that of the requisite buildings £400. In the year 1870 this signal was sounded during 122 hours, at a cost, exclusive of wages, of about 14*s.* 3*d.* per twenty-four hours of work.

Dr. J. H. Gladstone said that the Author had taken a comprehensive view of the subject, and as far as he was aware had touched upon every matter hitherto done with regard to it. While serving some years ago upon the Lighthouse Commission his attention, and that of his colleagues, was directed to the same question. They then found fog-signals in use that were of very doubtful value, and they had a good deal of representation made one way or the other as to the possibility of these fog-signals being improved. The matter had also been brought under the attention of the British Association for the Advancement of Science, and by that body before the notice of the legislature. No public body had shown a readiness to enter upon the proposed investigation, but certain experiments had been made. The Author had not described the experiments upon the conduction of sound through water, though it was stated that that idea had been thrown out. Experiments were made by Sir Charles Wheatstone and himself in the water system at the Polytechnic Institution, and afterwards by Sir Charles Wheatstone in the Thames, and by himself at sea. No doubt sound would travel very far through water; but water modified the character of the sound considerably. A musical note would not travel far, and all that was heard was a thud or tick. Thus there was difficulty in distinguishing between one sound and another. Long

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bars of steel struck with a hammer gave a volume of sound which was heard at a considerable distance, and he noticed that when the weather was rough the sound of the waves upon the shore and the rolling of the shingle could be heard at a long distance. He thought it might be useful in some cases to listen under water in the neighbourhood of a rocky or pebbly shore during a fog. They, however, came to the conclusion that it was useless to pursue that inquiry further, and that fog-signals should rather be made through the air than through the water. A difficulty in the use of sound was this—and it was touched upon in the Paper—that fogs deadened sound very materially; but the evidence was very contradictory on that point. In a fog on land it was difficult to hear the passing of carriages or noises at a short distance; and so in a fog at sea these signals found a difficulty in penetrating through the fog against which they were intended to be a protection. Nevertheless there was evidence that gongs and bells had been heard at considerable distances during fog; it might be that the air was tolerably homogeneous, or that the fog laid upon the surface of the sea, and that at a short distance above it there was clear atmosphere, and so the sound passed freely along this clear air and easily penetrated the thin layer of fog above the vessel. This in a great measure accounted for the variable distances at which the signals were heard; and it showed the difficulty of experimenting on fogs, when it was not known that any two fogs were of similar altitude. Besides the question of fog-signals for lighthouses, and especially for rock lighthouses, and fog signals on light-vessels, there was the important question of fog-signals on board ship. It was necessary in a fog that a ship proceeding on its way should inform neighbouring ships of its presence, and a signal of a powerful kind was requisite for this purpose. In the Royal Navy, when a fleet of vessels was at sea, and one ship wanted to communicate with another through a fog, the ordinary practice was to fire a gun. At the present time the ships were furnished with immense guns, which could not conveniently be fired as signals; there were besides very small guns which produced but little sound; between the two there was nothing which could penetrate the fog satisfactorily; and the steam power on board the ships was not of sufficiently high pressure to produce the various sounds emitted by the Vernon Smith machine. Fog-signals were more necessary now that steam had to a great extent superseded sailing vessels. If a fleet of sailing ships were overtaken by a fog all they had to do was to lay-to; with furled sails they remained pretty much in the same relative position one to

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another, the current being in the same direction and the wind having an almost equal effect upon all the ships. When the fog lifted, although they might be miles away from the place where they lay-to, they were generally within sight of each other. But steamers might be supposed to be moving at the same speed, and yet a difference of a quarter of a knot an hour might be found to have defeated all calculations when the fog lifted; instead of the whole fleet being together, the vessels might be scattered asunder out of sight of one another, and the consequences might be serious if an enemy were near. He thought the necessity of making experiments on this subject, and of employing the best fog-signals, could not be too strongly impressed upon all who had the direction of these matters. Fog-signals on board ships should be cheap, and at the same time effective and easily worked. Experiments were wanted to determine the kind of sound which penetrated best through a fog, and whether it was best to have a sudden, sharp, rattling sound or a consecutive sound; whether revolving sounds were desirable, or whether a steady sound in one particular direction was preferable. It was possible to steer by sound to a certain extent; this also should be borne in mind, as fog-signals were of great value in cases of vessels wishing to make a harbour.

Mr. E. A. Cowper said with regard to the practical application of an efficient fog-signal, he agreed that it was highly desirable that satisfactory experiments should be made on a large scale, and it was fortunate that a properly constituted Board existed for the purpose of establishing lights on the coast, and superintending the lighthouses and maintaining them in proper order. But he was sorry to say that that Board did not appear to be very energetic in endeavouring to introduce such improvements as bid fair to increase the safety of human life and property. He spoke on this subject from practical experience. In 1860 he was consulted by a committee of the Elder Brethren of the Trinity House, as to the best mode of ringing a large 2-ton bell at the South Stack light-house. It was evident that power had to be obtained for the purpose, and although it might have been produced from the wind, or from the motion of the waves, or from steam, and then have been stored up, by the elevation of a weight, it was thought simpler to let a man wind up the weight. He suggested at the time that the most efficient apparatus would be a plain portable boiler of 10 or 12 H.P., and either a steam-whistle or trumpet inside a gigantic speaking trumpet. Captains of vessels spoke from ship to ship at a considerable distance with a good speaking trumpet,

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and if that was simply imitated on a larger scale, by putting a whistle with 70 lbs. or 80 lbs. pressure of steam into a trumpet 20 or 30 feet long, he believed the sound would be carried 10 miles in clear weather, and at least half that distance in a fog, which was as much as was required. He was requested to get his views confirmed, if possible, by the late Dr. Faraday, the scientific adviser to the Trinity House, and he applied to Dr. Faraday on the subject, who at once advised that £200 should be spent in trying the apparatus practically; but this the Trinity Board declined to do, and as he did not feel inclined to spend that sum for the benefit of the Trinity House, the matter dropped.

Since that time a very similar instrument had been carried out, though on a smaller scale, by Mr. Vernon Smith, at St. John's, Newfoundland, and it was found that the sound could penetrate a distance of 7 miles in calm weather, and of 4 miles in stormy weather. It would seem that this steam whistle was far more effective than a gong or a bell, because the sound was of a more distinctive character than that produced by those instruments, so that it penetrated better through a fog. When he invented the fog-signal, now universally used on railways, his idea had been to produce a totally different sound to that of any noises connected with the train, and the 'bang' made by the explosion of a fog-signal was so different from the rattle of the train that it was distinctly heard. Carrying out the same idea, he thought he had produced a sound by means of a steam-whistle or trumpet, which was as effective, and as distinctive, at sea as a fog-signal on land. Of course it required either that the boiler should be kept warm in foggy weather, or that it should be made something like a fire-engine boiler, having great surface and very little water in it, and then the steam could be got up in a quarter of an hour or less, and that without any machinery or engine, because no expensive machinery should be used in an apparatus of this kind. He considered that the complication of a caloric engine and air pumps was unfitted for this purpose, seeing that the caloric engine had been generally condemned, by all the best mechanical engineers, as a machine by no means to be depended on. He thought a simple boiler and whistle, or trumpet, placed inside a speaking trumpet 20 to 40 feet long, was all that was required. He had proposed to make the speaking trumpet revolve similarly to the light, and then the mariner would know where he was. He felt sure that this idea would be carried out eventually, as it was the only certain means of penetrating a fog efficiently to the necessary distance; but he deprecated small experiments, which only

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ended in a failure of attaining the end in view, viz., the safety of the mariner.

Mr. J. N. DOUGLASS remarked that although the Trinity House had now much more efficient instruments in use than formerly, still it could not be said that the fog-signal was yet quite a trustworthy instrument in all states of the weather. A fog-signal to be perfectly reliable should be heard distinctly at a distance of 2 miles at least against a gale of wind. He thought the most efficient instrument was the trumpet of Professor Holmes. It was, no doubt, a more perfect signal than a gun. It emitted a very distinctive sound, and admitted of any desired arrangement of the duration of the blasts and the intervals between them, for the purpose of giving each station a distinctive character. He thought the arrangement of Professor Holmes for blowing the trumpet at a distance from the machinery was an important improvement in respect of lighthouses. It was now arranged that the trumpet, which was self-acting for regulating its blasts, could be blown at any distance in advance of the lighthouse, so that it might be placed in the best position and elevation on a cliff for sending its sound seaward. In no case should the signal be placed at an elevation of more than 50 or 60 feet above high water, if this was found to be quite out of reach of the heaviest seas. Mr. Cowper had alluded to the instrument which he brought under the notice of the Trinity House authorities a few years ago, and had complained that they did not spend £200 in trying it; but the Trinity Board and similar bodies could not spend the public money in trying every invention that was brought before them. He considered the caloric-engine was much better adapted than the steam-engine for fog-signals at lighthouses. In many cases rain-water only was available for supplying a steam boiler; and there was increased risk with a steam-engine, especially in the hands of lighthouse-keepers not experienced in working it. He thought it would be found that the most effective fog-signal was a trumpet worked by the caloric-engine, the cost of which, with the parts duplicated, would be from £800 to £1000. In every case he preferred that the machinery should be in duplicate, and this had been done with all those erected by the Trinity House. If any breakdown occurred, or repairs were required, to either machine the other was worked while the necessary repairs were being executed.

Mr. VIGNOLES, President, inquired whether Mr. Douglass had any suggestions to make for intensifying the sound of these signals. The machine he had referred to could be heard at a dis-

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tance of 2 miles in rough weather; whereas the best signal of other descriptions could not penetrate farther than ½ mile in a storm.

Mr. DOUGLASS said, he was not aware of any better means of intensifying the sound than that adopted in Professor Holmes' instrument, which gave an equal sound over an arc of 90°. Such a trumpet should either be rotated, giving a continuous blast similar to the flash of a revolving light, or one or more trumpets might be so fixed as to send their blasts nearly uniformly throughout the arc to be sounded over. The rotating trumpet of Daboll was defective in this respect, as in rotating it was sounded at intervals; consequently, each successive blast could not reach every mariner with the same power. By duplicating the apparatus the power of the signal could be doubled when required to blow against a strong wind. This he considered a point of great importance. He had found little difference in his own experience in the travelling of sound either in foggy or in clear weather. He had heard distinctly in a fog at the Smalls Rock, Bristol Channel, guns fired in Milford Haven, 25 miles off. In some states of fog he thought signals could be heard nearly as well as in clear weather, but he considered that the question of sending sound against a strong wind was one of much more serious consideration than that of merely transmitting it through fog. Fog varied much in height. In some instances it was at a considerable elevation, in others it was close down upon the surface of the sea.

Mr. VIGNOLES, President, mentioned an instance of a lighthouse on the west coast of Ireland, in which the light was not seen in a fog on account of the elevation at which it was placed. When placed lower down it could be seen.

Mr. DOUGLASS remarked that it was the general practice to limit the elevation of lighthouses to from 100 to 150 feet for the purpose of clearing fog; when higher than 150 feet they were frequently capped with fog.

Mr. MURRAY asked what was the situation of the fog-signal at Aberdeen, and its height above the surface of the sea?

Mr. A. BEAZELEY said it was at Girdleness, near the lighthouse, the whistle being 26 feet above high water of spring tides.

Mr. E. A. COWPER asked Mr. Douglass whether there was, to his knowledge, proof of a caloric-engine used for this purpose during the last thirty years without breaking down, even supposing the parts to be in duplicate?

Mr. DOUGLASS, in reply to Mr. Cowper, said Ericsson's caloric-engine had been employed at Dungeness and at St. Catherine's Head for the last four years, and neither of them had broken down.

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Mr. A. Beazeley, in reply upon the discussion, said that the subject appeared to be very little known; and, the more it was looked into, the more apparent became the fact that the evidence as to the effect of fog on sound was extremely conflicting. There were instances on record in which certain instruments were heard at considerable distances, and it might thence be inferred that such instruments would always be effective in fog; but there were other instances in which the same instruments had been heard at only short distances. This conflict of evidence he considered to be a reason why a properly-conducted series of experiments was requisite, in order to obtain some definite standard of measure of the power of various fog-signals, and of the intercepting or modifying effect of fogs. It had been remarked that a special trial of every new invention would lead to no practical result except the useless expenditure of money. In that opinion he entirely concurred. It was not random and desultory experiments of that nature that were required, but a broad, comprehensive, and scientific inquiry into the whole subject.

Mr. Vignoles, President, remarked that a bet was once made between two men as to whether a dustman ringing a bell on the top of Lambeth Church Tower could be heard at Battersea Bridge: on trial the bell was heard, advantage being taken of tide and wind.

With respect to fog, in some countries it did not exceed an elevation of 50 or 100 feet, but at that height was cut off, and a man ascending a hill came into a perfectly clear atmosphere above the level of the fog cloud. These were circumstances which must be taken into account in making experiments on this matter. There ought to be no difficulty in having a boiler with sufficiently high pressure to sound fog-signals on board a ship. The difficulty was with regard to fog-signals on shore, and Mr. Douglass had suggested that the only means was, if there was no lighthouse, to have them as near the shore as possible. Whether a fog-signal would be sufficient to overcome the din of the storm in the sailor's ears was another question.

Mr. W. H. Delano remarked, through the Secretary, that a steam boiler was far simpler than a hot-air engine, and needed much less repairs. Hot-air engines had no reserve power, whereas from the great elasticity of steam a pressure proportionate to the density of the fog, or the force of the opposing gale to be penetrated, might be easily obtained. Steam-whistles on Mr. Cowper's plan might be erected at different stations on the coast, each station

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being provided with an anemometer; the pressure of the steam could then be adjusted to the pressure of the wind or the density of the fog, and penetration assured. The monster trumpet should be fixed on a swivel-head. A screw steamer was now lying in the Thames, the 'Fairy Dell,' from the port of Sunderland, the boilers of which, on Howard's water-tube principle, were regularly worked at a pressure of 150 lbs. to the square inch. A jet of such steam through a Cowper whistle would enable a captain to make his signals heard in a fog; and an admiral might thus signal a whole fleet. This fact disposed of the statement, that such signals could not be used at sea on account of the low steam pressure used in marine boilers.

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March 21, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

No. 1,231.—"Description of a Wrought-iron Pier at Clevedon, Somerset."[1] By JOHN WILLIAM GROVER, M. Inst. C.E.

THE original Act for constructing a wrought-iron pier at Clevedon, in Somersetshire, was obtained in the parliamentary session of 1863-4, but the works were not commenced until the autumn of 1867; little being really done upon the ground until the spring of 1868, which was singularly unpropitious for the erection of sea-structures. However, by the end of November, 1868, the work was practically completed, and it has since withstood very severe storms without injury. The coast of Somersetshire is at this spot composed of dark cliffs of magnesian and mountain limestone, blocks of which, some of them in boulders of 40 tons or 50 tons in weight, lie scattered upon the beach. These are embedded in mud and slush near the shore, and amongst them lie the fragments of an ancient stone mole now completely destroyed. The site selected for the works was comparatively free from boulders and mud, being occupied by a thin promontory of rock, with a tolerably level bed from left to right, and dipping seawards at an angle of from 12° to 8°. Beyond this came a bed of soft mud, covering, at a depth of 13 feet, a substratum of hard red clay, in which boulders and stones were embedded. At the pier-head the foundation consisted of a ridge of sand, below which, at a depth of 2 feet, the bed of hard red clay was met with. The chief difficulty to be contended with was the great rise and fall of the tide, amounting at average springs to 45 feet; and the Bristol Channel being only 9 miles across at this point, the velocity of the current of the advancing or retiring waters was very great, in extreme cases moving about 5½ miles in the hour. Clevedon is also exposed to the south-west, whence the prevailing winds blow up the channel. These circumstances, added to the bad holding nature of the ground, caused much delay, the stages being frequently destroyed,

[1] The discussion on this Paper was taken in conjunction with the two following ones on somewhat similar subjects, by Mr. Conybeare and Mr. Maynard, and was continued over portions of two evenings.

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and the boats and barges moored outside being driven ashore and wrecked.

The structure comprises, first, an approach 180 feet long, and 20 feet wide, on a gradient of 1 in 10, the face being built of carefully squared masonry; and passing through the masonry at an angle of 45° are two mooring bolts, 2 inches in diameter, run in the rock with lead. There are also a toll-house, gates, and a dwelling for the pier-master. In the next place, the body of the pier consists of eight pairs of girders, in spans of 100 feet each, having arch pieces below, the whole supported on wrought-iron piers. The girders are 3 feet 6 inches deep, and the bottom and top flanges are 18 inches wide. The girders are continuous for the entire length of the 800 feet, and each girder weighs about 7 tons. The webs are of plates $\frac{3}{8}$ths and $\frac{1}{4}$th of an inch thick; the bottom and top flanges being each composed of $\frac{9}{16}$-inch plates averaging 20 feet long, the covers being 2 feet long, $\frac{9}{16}$ths of an inch thick, and 1 foot 6 inches wide. The vertical joints in the web are generally made with T irons, 5 inches by $2\frac{1}{2}$ inches by $\frac{3}{8}$ths of an inch, and these are carried above the girders to support the planking, which forms the backs of the seats, the seats themselves being carried upon the top flanges of the girders. The rivets have a uniform pitch of 6 inches, and are $\frac{3}{4}$-inch in diameter. The L irons which connect the web to the bottom and top flanges are 3 inches by 3 inches by $\frac{3}{8}$ths of an inch, and are jointed by covers 2 feet long, $2\frac{1}{2}$ inches by $2\frac{1}{2}$ inches by $\frac{3}{8}$ths of an inch. The main longitudinal girders are placed at a distance of 16 feet 6 inches apart from centre to centre, and are connected by transverse girders 20 feet 6 inches apart, composed of angle irons 2 inches by 2 inches by $\frac{1}{4}$th of an inch, and $\frac{1}{4}$th-inch webs 9 inches deep. From the ends of the transverse girders, and below the deck, run flat bars horizontally and diagonally, each bar being 3 inches by $\frac{1}{2}$ inch, thus forming, with the main longitudinal girders, one large lattice girder, 16 feet 6 inches deep, laid on its side. This bracing is for the purpose of giving lateral continuity, which is also assisted by the close deck-planking, 3 inches thick, laid on joists, 10 inches by 5 inches in section, 3 feet 5 inches apart, and having a camber of 4 inches.

The piers are composed of four wrought-iron piles, each made of two Barlow rails, weighing 80 lbs. to the yard, rivetted back to back, and presenting a total section for each 100 feet span of 64 inches. These piles are built with a rake of 1 in 10 fore and aft, and they are braced together transversely by diagonal tie-rods from $1\frac{3}{4}$ inch to $2\frac{1}{8}$ inches in diameter, screwed through slings at

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both ends; the distance pieces being booms of four angles, iron having small cast-iron Maltese cross struts between them, fastened with $\frac{7}{8}$-inch and 1-inch bolts. The angle irons in these booms are $3\frac{1}{2}$ inches by $3\frac{1}{2}$ inches by $\frac{1}{2}$ inch, and 4 inches by 4 inches by $\frac{1}{2}$ inch, and are curved at each end to embrace the four piles. These booms are 20 feet apart on an average vertically. Sideways in elevation the piles are kept apart by T irons, 6 inches by 3 inches by $\frac{1}{2}$ inch, and are braced with tie-rods from 1 inch to $1\frac{1}{4}$ inch in diameter. At the top the Barlow rails are curved to form arches longitudinally and transversely, the latter being carried round to meet each other and joined together under one strong distance girder at each pier. Longitudinally the Barlow rail arch pieces have $\frac{5}{8}$-inch plates on the backs, and the principal spandrel-piece above them is of L iron, $4\frac{1}{2}$ inches by $4\frac{1}{2}$ inches by $\frac{1}{2}$ inch, with a web plate 9 inches by $\frac{1}{2}$ inch, all being intimately connected with the web of the longitudinal girders on the top. Though the two piles on each side are separate and independent at their lower extremities, they unite and are clustered together at the top, a $\frac{1}{2}$-inch plate running up between them.

The foundations of the piles, that is, all parts below extreme low water, are formed of solid wrought-iron stems, 5 inches in diameter, shod with cast-iron screws, 2 feet in diameter; these were screwed down till a $4\frac{1}{2}$ inches rope, passing round 6 feet capstan bars, parted with the strain. They penetrate the ground to depths varying from 7 feet to 17 feet, and though made with a thread 5 inches in pitch, seldom descended more than $2\frac{1}{2}$ inches or 3 inches in a turn. The solid pile stems are connected to the Barlow rail piles by cast-iron shoes.

The iron work below extreme low-water level was coated before immersion with coal tar and naphtha; that above was painted with red lead. Where the foundations near the shore came directly upon the rock, holes were jumped, and a 4-inch wrought-iron bar was inserted, and secured by a jagged key. A shoe to receive the Barlow rail was fitted and keyed on this, and the remaining space was caulked with iron cement.

The length of the longest pile was 76 feet. At extreme low water there is a depth of water of from 6 feet to 7 feet at the head, and at high-water of about 52 feet. The level of the deck above extreme high water is 16 feet, so that the height of the deck above the ground at the pier-head is about 68 feet. This is the only pier between New Passage and Land's End which commands the sea at extreme low water.

The head occupies a space of 50 feet by 40 feet, and consists of

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one pier similar to those at intermediate distances, the others being formed with Barlow rail-piles, weighing 70 lbs. to the yard, placed upright and connected together with rolled iron joists, 10 feet apart vertically. There are five lower stages or decks, for landing at different states of the tide, placed 10 feet apart, and connected by stairs. The whole of the head is braced diagonally and horizontally with booms and tie-rods, so as to resist the action of the waves and the shock of steamers. It is also protected by fender piles, 8 inches square, which are secured to each horizontal girder by bolts $\frac{3}{4}$ inch in diameter. It was of the greatest importance that considerable rigidity should be given to this lofty erection, exposed as it is at high tide to strong currents of water, besides the fury of the gales and the shocks from steamers, and the straining of their ropes in holding on; and not much vibration is perceptible upon it during a storm. The stairs are formed with steps rising 6 inches, and there are eleven treads; they lead to the five lower decks or landing platforms, so that the total descent is 50 feet to the lowest stage. Spaces of 2 inches are left between the battens of the steps and the stages, and they are chamfered off to allow the waves to pass through. Notwithstanding these precautions, some of the steps have been carried away in a storm; and, where cost is no object, the Author would recommend in future works of this kind, that open cast-iron treads and risers should be employed.

In erecting the pier, a floating-stage was first constructed with six galvanised cylindrical corrugated iron pontoons, capable of lifting a weight of $1\frac{1}{2}$ ton each, fixed in a rectangular frame of light trellis girders, having on the top planking 3 inches thick; thus forming a deck 20 feet square, and 2 feet out of water. But, owing to the ruggedness of the shore, this stage was found inapplicable, and a barge was afterwards used. This barge was 68 feet long, 14 feet beam, and decked; but it was wrecked in a gale of wind in February, 1868, breaking from its moorings on the ebb tide. After drifting a distance of 5 miles, it returned on the flood, and passed within 20 feet of the pier, of which a length of 400 feet was then erected, and to which it was imprudently made fast by a rope, of $4\frac{1}{2}$ inches, which however parted with the strain. Another decked barge was then procured, and stood the weather till the pier was finished. The boats used were of the class known as whale-boats, with an air-tight compartment at each end. They were of iron, and were 28 feet long, and 7 feet beam. A flat-bottomed boat was employed for launching, on account of the roughness of the beach. The first

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stage for screwing in the piles consisted of a framework of timber built to the proper batter both ways. A centre-rod having been fixed at the pier to be commenced upon, this stage was floated out, and as the tide left, it was grounded in its proper position by a guide to the centre-rod. It was then weighted with large stones to keep it from floating away as the tide returned; the piles could then be pitched without any further setting out. As the work proceeded, and the water became deeper, a stage of driven piles was substituted. This consisted of eight piles, 5 inches by 5 inches, driven in about 6 feet; two walings were spiked on each side, 7 feet apart; on these the guides were fixed to pitch the piles, being accurately set out to the batter. The stage was braced with iron, partly for the sake of weight, and partly to diminish the area exposed to the sea. The pile being pitched, a capstan was keyed on with three steel wedges; the length of the capstan-bar was 5 feet, and the power was applied by an ordinary crab. At the periphery of the pile it required a strain of about 27 tons to turn it, or nearly 1-ton strain at the ends of the capstan-bars. The four piles composing the foundation of one pier being fixed, the next operation was to lift the pier legs into position, after they were rivetted together on the beach, and floated out by two pontoons. Being towed into proper position at high water, they were hung at one end in the tackle from a derrick or mast, 90 feet high, one pontoon being unlashed; as the tide fell the other went down, and the pier-leg was left perpendicular at low water, and ready to ship on to the connections. When the legs were fixed, and the bracing, &c., put in, the pier was ready to receive the main girders. These were fixed by two methods. The first was by launching them over a span already completed; the girder was run out on rollers and being supported by a derrick, placed midway in the span, with two sets of tackle;—one holding the weight, the other being shifted back to give the necessary strain forward. This operation was found to be dangerous, and occupied three or four days for each girder. The second method was found more convenient and expeditious: thus, the girders were placed on the shore and floated out by eight pontoons, beneath their proper span; they were then lifted at one end by the derrick used to raise the piers, and at the other by a jib rigged out at the extremity of the span already completed. By this arrangement the jointing of the girder could be effected whilst it was suspended by the tackle. The pontoons were of course cut adrift before the raising was commenced.

On one occasion the whole operation, including floating the

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girder, took only an hour and a quarter; but this was under a favourable condition of the weather; sometimes several days were employed in getting the girder into position. The number of men actually occupied in the most successful launch was fourteen, but generally thirty or forty were not sufficient. No difficulty occurred in fixing Nos. 1 and 2 piers, as they were on sound rock, known locally as bastard freestone, but geologically as magnesian limestone, which in this district directly overlies the carboniferous limestone. This series overlies the coal measures with which is found the blue Pennant sandstone, a rock peculiar to this district and South Wales, and of which the abutment of the pier was built.

The raising of the derrick-pole was a matter of difficulty, the surface of the lower beach being chiefly mud. It was first put up with an anchor weighing 6 cwt., on each of the two broadside guys, the other two guys, fore and aft, being made fast to the work; but as the tide grew strong one anchor dragged, and let the derrick down. Two anchors, weighing 6 cwt. each, were then tried, with wooden piles driven against their flukes, but this was of no avail; finally, large pieces of rock were floated out to the proper position, and then sunk; and to these the guys were attached by a chain, fixed to a ring-bolt.

No. 5 span and No. 5 pier were built in ten days; but the entire work took fifteen months; in some cases as many as three months per span, when the weather was unfavourable. The average number of men employed was about sixty.

The metal in the girders was calculated to bear tensile and compressive strains of 5 tons per square inch. The strain on the piles in compression was 2 tons per square inch when wholly loaded.

The structure is made to carry 1 cwt. to the superficial foot of available deck surface, being 16 cwt. per foot run, or 80 tons per span of 100 feet. Specimens of all the iron employed were carefully tested at the works; and one span of the completed structure was loaded in the centre with 42 tons of stone, placed in such a manner as to be equal to a distributed load of 80 tons on the span, exclusive of its own weight. The deflection in the centre with this weight was $1\frac{1}{4}$ inch, the calculated deflection being 1 inch, and it was only temporary, the mean permanent set being about $\frac{2}{3}$ths of an inch. The weight of wrought iron was under 370 tons, and of cast iron 7 tons; and the entire cost of the works was under £10,000.

The contractors for the pier were Hamilton's Windsor Ironworks

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Company, of Liverpool, and for the approaches Mr. Ambrose Oliver, of Hitcham. The works were designed and carried out by Mr. R. J. Ward, M. Inst. C.E., and the Author of the Paper, as joint Engineers; Mr. J. Wallace Peggs being the resident Engineer.

The Paper is illustrated by a series of diagrams, from which Plate 10 has been compiled.

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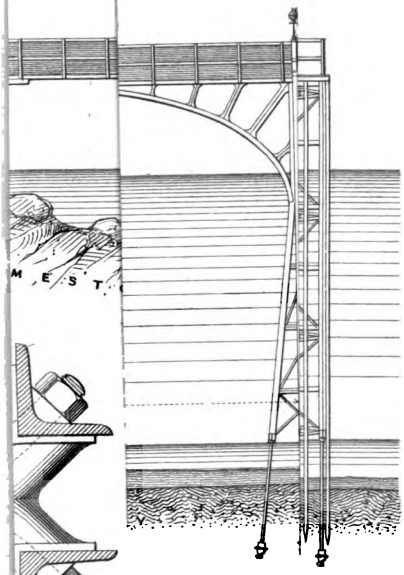
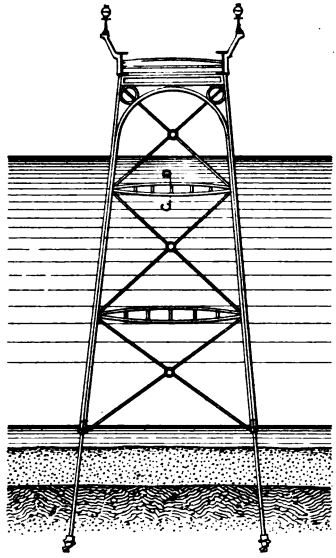
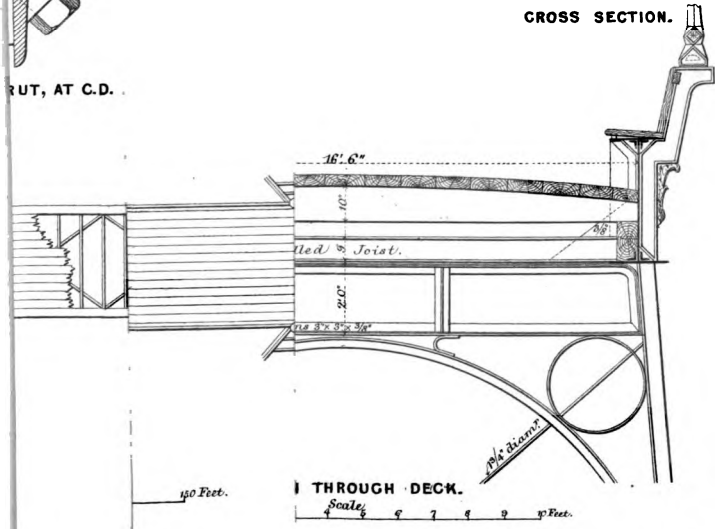
Fig: 4.





CROSS SECTION.

RUT, AT C.D.



150 Feet.

THROUGH DECK.

Scale 1 0 1 2 3 4 5 6 7 8 9 Feet.

KELL, BROS LITHRS CASTLE ST HOLBORN.

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No. 1,233.—"Description of Viaducts across the Estuaries on the Line of the Cambrian Railway."[1] By HENRY CONYBEARE, M. Inst. C.E.

THE coast line, which this section of the Cambrian railway follows for nearly its entire length of 86 miles, is indented by numerous estuaries, which are crossed by viaducts having an aggregate water-way of upwards of 5,000 feet.

Most of these estuaries are shallow, being generally dry at low water, and, when such is the case, timber staging has been employed. Two of the viaducts have been provided with opening spans—that over the Dovey, above Aberdovey, which has a span of 35 feet, and that over the Mawddach, at Barmouth, which has an opening of 36 feet.

The reasons for using timber, instead of wrought iron, for bridging the shallower of these estuaries were entirely financial. Designs were prepared, in the first instance, for wrought-iron superstructures, resting on cast-iron piers, for all the estuary crossings; but these were objected to on the score of expense, and there were special circumstances which rendered the use of timber less open to engineering objections, and more advantageous in a financial point of view than is ordinarily the case; for the nature of the sites allowed of the timber in these structures being constantly observed, and of any defective pieces being readily replaced. The relative cost of timber staging of this description as compared with that of an iron structure is, under ordinary circumstances, as 1 to 3, and in this particular case was as 1 to 4, for the Contractor brought the timber by sea direct from the Baltic, the ships being unloaded on the works, which rendered the cost of the material, as put into the works, exceptionally low; whereas, on the other hand, the cost of an iron structure, constructed in such an out-of-the-way locality, is always exceptionally high.

The great aggregate length of these viaducts made the saving attendant on the use of timber instead of iron very material, especially when the local character and objects of the line are

[1] The discussion on this Paper was taken in conjunction with the preceding and the following ones, on somewhat similar subjects, by Mr. Grover and Mr. Maynard respectively, and occupied portions of two evenings.

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taken into consideration; for its course is along a barren coast skirting the highest hills in Wales, and open throughout to the competition of water carriage, its object being to render accessible a succession of sites for bathing towns, unsurpassed in picturesque beauty and waterside capabilities by any hitherto built upon in Great Britain. The traffic of such a line must necessarily be unproductive at first, and take many years to develop. These circumstances rendered a reduction in first cost especially important, for the three-fourths of the first cost of an iron viaduct saved by substituting a timber structure would double itself in the fourteen years which would elapse before the timber structure would require to be replaced, and would then amount to half as much again as would be required to replace the bridge in iron; and such an iron structure would then be better by the amount of fourteen years' wear and tear than if it had been erected in the first instance.

The construction of the timber portions of these viaducts presented nothing remarkable, excepting that the unusual velocity of the tidal currents would have uprooted the timber piles, driven, as they were, in sand, unless the stoning of the surface immediately followed the driving of the piles.[1]

In all cases, however, the viaducts across the low-water channels of the estuaries were permanent constructions, with wrought-iron superstructures resting on cast-iron piers.

The viaduct over the tidal water of the Dovey has an opening span of 35 feet, on the principle first used by Mr. Brunlees, M. Inst. C.E., in the viaducts at Morecambe Bay. The channel is not more than 3 feet deep at low water of spring tides; and as it was important to complete the bridge as rapidly as possible, the Author employed ordinary piled foundations, fixing a cast-iron splice at the top of each timber pile, and driving it with it, so that the whole of the timber should be sunk in the bed of the channel. So placed, timber piles will last as long as iron, and they afford a cheaper foundation and one more rapidly executed than screw-piling. A similar expedient was adopted in the construction of the fenders of the Barmouth viaduct; the depth of water in which these were erected was 54 feet at spring tides, but this has since been somewhat reduced by the stoning of the channel. As the harbour is free from worms, timber supports were used in the fenders, precautions being taken that all portions of the structure which were not constantly immersed, and which were therefore exposed to decay, through alternations of moisture and dryness, should admit

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxi., page 288.

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of being constantly watched and readily replaced when found defective. The piles employed were 14 inches square, driven to a little below the lowest low-water level, and cast-iron splices were interpolated between the tops of the piles and the timbers of the upper portion.

The construction of the deep-water portion of the viaduct over the estuary of the Mawddach, at Barmouth, was rendered difficult by the peculiar nature of the foundations, and the extraordinary velocity of the current at certain times of the tide. The tidal waters of this estuary extend inland about 10 miles to near Dolgelly. The southern margin, along which the Dolgelly branch of the Cambrian railway takes its course, is formed by the northern and more precipitous flank of Cader Idris, and the northern margin is almost equally mountainous. At the site of the viaduct, the water-way of the estuary is contracted to one half its width above, by the projection from Cader Idris of a craggy promontory, called Figle Fawr, and the width of the tideway is further reduced at this point by some rocks, named Cerrig-y-Gorlwyn. These contractions rendered the current in the neighbourhood of the viaduct abnormally rapid at certain times of the tide, the velocity being estimated at 9 knots an hour. The Author has not tested the accuracy of this estimate, but some idea of the force of the current may be gathered from the fact, that it once overpowered a tug-steamer and carried her through the timber portion of the viaduct, which she breached, although the water is shallow, and the current not nearly so rapid in that part as in the deep-water channel near the northern shore.

The deeper portion of the channel, on the line of the viaduct, is close to the northern shore, and the northern bank, which is of basalt, descends rapidly into deep water. The piers No. 1 and No. 2 rest on the rock. Immediately south of the pier No. 2 the rock shelves down precipitously and disappears; and beyond this the bed of the channel is sand, constantly shifting with the current, and varying in depth from 2 feet to 8 feet. Below the sand is a bed of compact gravel, from 6 feet to 8 feet deep, and below this again a peat bog to a depth not passed through in the preliminary borings.

It was required that the viaduct, by which the estuary was traversed, should have a water-way of 2,600 feet; that there should be an opening span of 35 feet clear between fenders in the deep channel, which at ordinary spring tides was 54 feet in depth; and that the viaduct should be for a single line of railway only, but that there should be a footway 9 feet wide throughout its length on

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its northern side, with recesses for seats every 40 feet, to serve as a promenade for commanding the view up the estuary, and to connect the existing town of Barmouth on the north with the site of the new town, which, it was expected, would spring up along the sandy bay immediately south of the viaduct.

The first question the Author had to consider was the nature of foundations most suitable to such conditions. It was obvious that the piers No. 1 and No. 2, which rested on rock, must be cylindrical, though considerable difficulty was expected in sinking masses, exposing so large a surface, in so rapid a current, in a position where staging was almost impracticable. For this reason cylinders were considered inapplicable for the other piers, and screw-piles were consequently determined on.

The area of the screw discs of the piles was calculated so as to make the stress exerted by the weight of the structure on each square foot of their surfaces about 2½ tons, which is about half their sustaining power; and these areas of bearing surface were afterwards supplemented by discs of from 4 feet to 4 feet 6 inches in diameter bolted on to the upper portion of the piles, and bearing on the stoning.

It was obvious that the thin crust of gravel was all that could be depended on to carry the bridge, for the sand that overlaid it was continually shifting, and the bed of peat below was of unknown depth; and whenever it had been met with at the surface in adjoining portions of the line, it had proved very unstable. The screw discs were therefore placed 8 feet up the pile, so as to take their bearing on the gravel, which the lower portion of the pile penetrated. As the crust of gravel was thinner than could have been wished, the weight was distributed as much as possible, and spans of only 40 feet were adopted, each pier being formed of six piles, with screw-discs 3 feet in diameter. The piles were 10 inches in diameter in the shallower portions of the channel, and 14 inches in the deeper.

In order to equalize the weight on the piles, and to distribute it over a larger area, the six piles of each pier were arranged in two groups of three each, the upper lengths of each group forming a tripod. By this arrangement the weight was distributed more evenly, and over a wider area than would have been possible if the six piles had been placed in only a single row.

In viaducts where the piles of each pier are arranged in a single row, a pair of balks is usually placed, one on each side of the row of piles, at the level of the stoning, and bolted together, thus affording an additional bearing surface, and distributing the

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pressure on each pier over a wider area. Similarly, at Barmouth, after the stone had thoroughly settled, its surface was levelled by divers round each pier, and discs of from 4 feet to 4 feet 6 inches in diameter, cast in halves, were bolted to each pile, so as to bear on the surface of the stoning. A slight additional depth of stoning was then added. Each of these discs presented to the stoning an area of from 12 square feet to 20¼ square feet, and formed the frustrum of a cone of resistance, whereby the weight of the pier was distributed over a larger area on the bed of gravel beneath.

The inconvenience inseparable from this mode of construction when applied in such situations is, that the piles of which each pier is composed, though sufficiently strong collectively, when the pier is complete, and all its constituent parts are firmly fixed and braced together, have little individual strength when erected singly, as they must be, and remain unbraced; and if stormy weather and high tides should supervene after the fixing of any pile or piles, and before they are permanently braced together, they are liable to be injured. So here, though the piles were temporarily braced to the staging, as effectually as was practicable, yet in such deep water, and in such a rapid current, it was found impracticable to secure absolute immobility in a stage constructed of an elastic material like timber. And on the occasion of a high tide, accompanied by stormy weather, on the 5th of August, 1865, the staging of No. 5 pier was damaged, and one of the piles broken off; on the 15th of the following month the broken fragment was recovered; and it was then found that the sandy bottom of this portion of the channel had been scoured off to the depth of 5 feet, and deposited in the deeper part.

On the 4th of March in the same year a high tide, accompanied by a gale of wind, broke one of the piles in No. 4 pier; and in a storm, which occurred on the 24th of the same month, the temporary staging of this pier was swept away, carrying with it all the piles which were not yet permanently braced. The temporary stage was refixed in a few days, and divers were set to work to recover the broken piles; but in a shifting bottom the discovery of such stumps is not very easy, and, when they are found, they are difficult to get out; and till this is done the new piles cannot be fixed. This accident caused nearly a month's delay in the completion of pier No. 4.

After the six piles composing a pier were screwed home, the horizontal straining pieces had to be fixed before the piles could be braced together; and when heavy weather came on in the interval,

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in the case of the deeper piers, the straining pieces were subjected to a considerable stress where they were bolted to the piles. Thus, during a storm that occurred on the 17th of April, 1866, some of the straining pieces of pier No. 3, which was still unbraced, were broken, though the piles remained uninjured; but the broken castings were quickly replaced by forgings, and the bracing and the pier were completed without serious delay. There was no instance of the slightest accident occurring to a pier after the bracing was once completed; and as soon as all six piles in a pier were screwed home the stoning was immediately proceeded with.

The cylindrical piers each consisted of two 6-feet cylinders, set with their axes 18 feet 6 inches apart, and firmly braced; thus making the base of the piers 24 feet, measured in the direction of the current. The cylinders of pier No. 2 are 56 feet in height from the rock on which they are bedded to high water of spring tides. They were bedded by divers, and were filled with concrete.

The sinking of these cylinders was commenced by Mr. Phillips for the Author in the autumn of 1865, and one of Messrs. Cochrane's most experienced foremen, Mr. Airy, was employed on the work; but the first attempts were unsuccessful. The cylinders were sunk from large barges moored as firmly as was possible in such a current; but the season was unusually stormy, and it was found impracticable, by the means employed, to steady the cylinder sufficiently against the current to fix it in its place. It was sunk 3 feet into the shifting sand, which happened at that time to overlay the shelving basaltic rock; but its position shifted with every high tide, and the divers were exposed to so much risk, that Mr. Phillips reported, on the 27th of November, that he was under the necessity of discontinuing the work until the ensuing spring. The cylinder was accordingly taken up, and the work suspended for the winter. In the spring of 1866 the sinking of the cylinders was resumed; the barges were abandoned, and the cylinders were sunk from a stage projected from the abutment, and supported by a trestle, heavily weighted with rails, resting on the rock on the abutment side of the cylinder. By the 17th of March the two cylinders of pier No. 1 had been sunk, and the levelling of the rock for their beds by divers was in progress; and by the 26th of April both were fixed and filled with cement to high-water mark. By the 31st of May the first cylinder of pier No. 2 was fixed, and filled with concrete to high-water mark, and the second was completed by the 30th of June. The cylinders were filled with concrete made with cement in a wet state. As soon as the divers reported that they were ready for filling, the resident Engineer, Mr. Taylor, went down

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in a diving dress, and when he had ascertained that they were properly bedded on the rock, the filling in was commenced.

The roadway was supported by trellis girders 40 feet in length and 4 feet in depth. The opening span is 47 feet between the points of support, contracted by the fenders to 36 feet; but the piers might have been so arranged as to give an available opening of 45 feet or 46 feet with the same drawbridge. The description of drawbridge employed for closing this opening was determined by the specialities of the foundation of the pier on which it rested, which was sunk in hard rock, shelving abruptly in a direction transverse to the axis of the bridge. It was obviously of primary importance to restrict the area of such a foundation as much as possible, and to dispose its area transversely to the axis of the bridge; and this consideration rendering a swing-bridge out of the question, there only remained the telescope or sliding drawbridge. Of this there are two varieties, the under drawbridge and the over drawbridge. The former was devised and first employed by Mr. Brunlees, and was that adopted by the Author for the opening span of the Dovey viaduct. In this arrangement the drawbridge is drawn back underneath the roadway of the adjoining superstructure, and has to be lifted into position and supported from beneath for the passage of a train. The over-drawbridge is less generally known. The Author is only acquainted with one previous instance of its being carried into execution, viz., at the Rhyl bridge, where the over-drawbridge is in two pieces, meeting in the centre of the opening span; a mechanical defect in its application to this particular example which does not affect the general principle of the arrangement. Irrespective of its general mechanical advantages of working more easily, and of being dropped into position, instead of being lifted into it and supported in it, the over-drawbridge was much better adapted to the requirements of the Barmouth site, in respect to foundation, than the under-drawbridge; for the latter requires at least a second row of piles for the support of its sliding drawbridge, and as this has to be withdrawn between and within the supports of the contiguous bay, its width is either restricted, which in this case would have interfered with the foot-bridge alongside the railway, or the intervals of the piles it slides between is unduly increased.

In the over-drawbridge, Fig. 8, Plate 11, while the details of the structure are given in Plate 11A, it will be seen from Fig. 8 that the roadway of the opening span is supported between two ordinary girders, which drop into their position when the bridge is closed. Each of these girders is pivoted on two wheels, one on each side,

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at C, which wheels, together with the girders they support, can be rolled back, when the drawbridge is tilted out of its closed position, along the rails B G′ I to I, thus leaving the opening span clear. To effect this withdrawal, a triangular prolongation D B F is affixed to the girders, having at its extremity a small wheel F, in contact with the under side of the rail F G H, of which the short length F G, nearest the opening, is bent up at right angles to the farther portion of its course G H, which is horizontal. The entire girder thus becomes a beam A C F, pivoted at C, about the centre of its length; and the arm C F is weighted, so as to reduce, within as trifling limits as may be desired, the preponderance of the arm C A. Along the underside B F, of the triangular arm, there is a rack working in a spur-wheel turned by hand, and situated just below G. When the span is to be opened by the withdrawal of the drawbridge A B, the latter is tilted up out of its closed position by bringing down the opposite extremity F of the beam to G; by this movement the double flanged wheel, or pulley F, is brought into contact with the underside of the horizontal guide-rail G H, and the rack on the underside B F of the triangular arm into contact with a spur-wheel at G′. In order to act simultaneously on the opposite girder, a similar spur-wheel is keyed on to the same axis, immediately beneath the rack B F of such opposite girder; and thus, by turning a winch, the whole drawbridge is rolled back into the position in which it remains while the bridge is open. To close it again, the winch is turned in the opposite direction till the bridge attains the position shown by the dotted lines, and it is then gently lowered into its bed.

In the Barmouth example (Plate 11A), the four wheels on which the drawbridge is rolled back are each 4 feet in diameter, and their axles $3\frac{1}{4}$ inches; but between the axle and its bearings are interpolated nine steel rollers, set in an annular cage of steel, which keeps them at their proper distance apart. By means of these large wheels, working in bearings, the friction to be overcome in running the drawbridge in and out is reduced to a minimum; and the distance between the centres of the wheels, 50 feet, is so great as to insure parallelism of movement. In these respects this arrangement possesses great advantages over the under-drawbridge, which rests and works on comparatively small wheels, and has to be lifted into its position and held up under the trains by eccentrics; whereas, the over-drawbridge drops into its bed by the effect of a slight and readily adjustable preponderance, and is lifted out of it and withdrawn with equal ease; but, as in all

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Fig. 3

PLATE II.

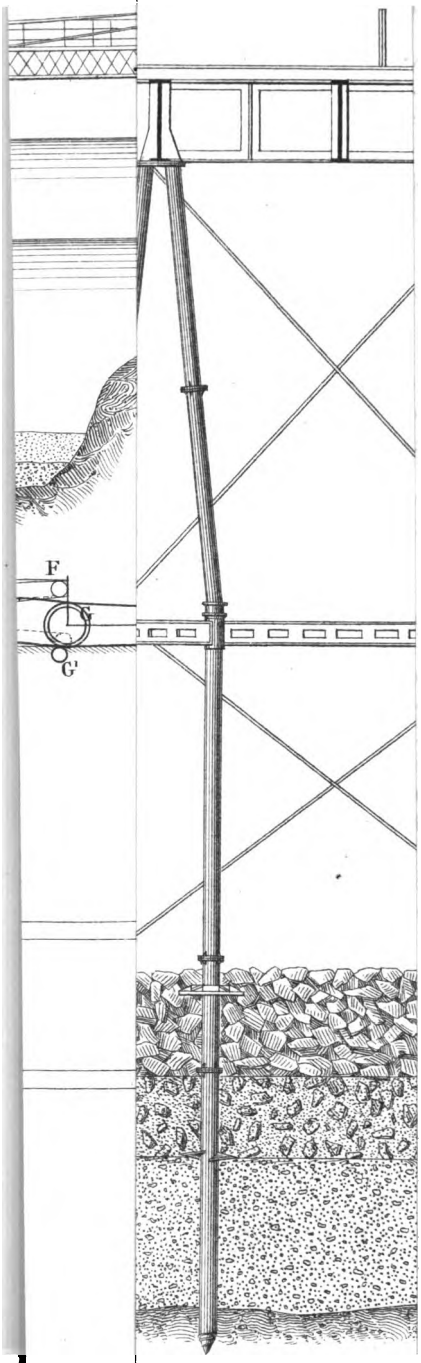
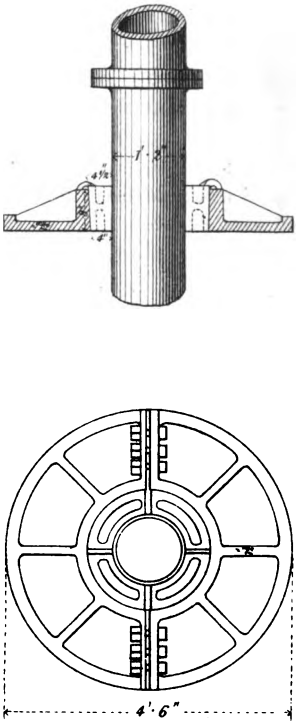


Fig. 6.



KELL, BROS LITHRS CASTLE ST HOLBORN.

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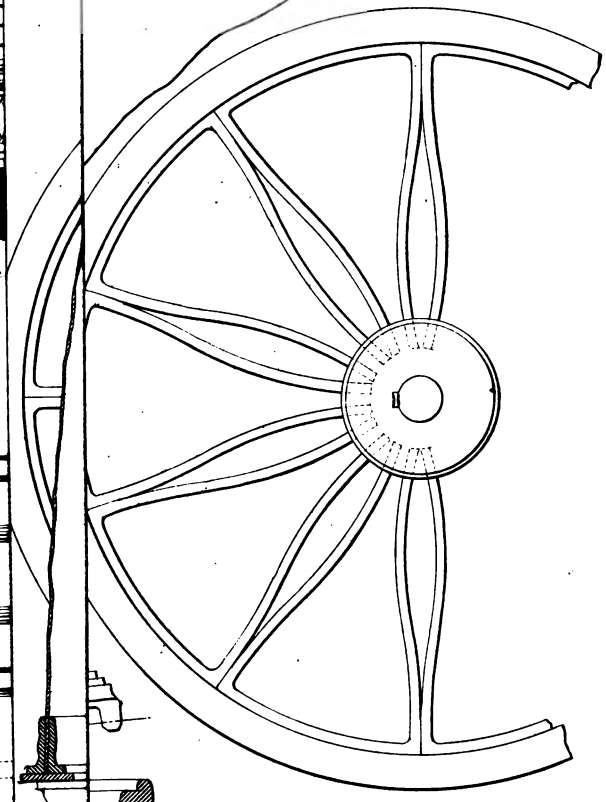
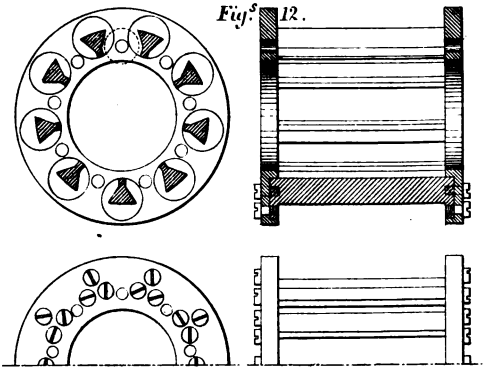
Fig: 11.





Figs 12.

FRAME FOR FRICTION ROLLERS.

Scale for Figs 12. 1/6 real size.

KELL, BROS LITHRS CASTLE ST HOLBORN

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drawbridge work, accurate workmanship and fitting are required to ensure its satisfactory performance.

In conclusion, it may be remarked that the difficulties experienced in the execution of the Barmouth viaduct were, in great measure, due to the circumstance of the work having been commenced in autumn instead of in spring. As regards the question of foundation, the screw-pile piers, when once fixed and braced, have answered expectations, and in this instance nothing better could be afforded; but in tidal waters the Author would prefer cylinders, filled with cement-concrete, compared with which screw-pile piers are, at their best, owing to their greater liability to accidental damage, but an expedient. He considers 6 feet too small a diameter for cylinders sunk, as at Barmouth, in a depth of 56 feet of water. The considerations that induced him to adopt so small a diameter in this case were exceptional, viz., the hardness and rapid slope of the rock, and the extraordinary force of the current: had it not been for the former of these considerations he would have enlarged the cylinders, at the base, to a diameter of 10 feet. As regards bridges with opening spans, his experience in both varieties has led him to the conclusion that over-drawbridges are preferable to under-drawbridges.

The Paper is illustrated by a series of drawings and diagrams, from which Plates 11 and 11A have been compiled.

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March 28, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

No. 1,283.—"The New Ross Bridge."[1] By HENRY NATHAN MAYNARD, M. Inst. C.E.

THIS bridge is situated on the river Barrow, at the town of New Ross, and connects the counties of Wexford and Kilkenny. It occupies the site of an old wooden structure, which was swept away by an accumulation of ice during the winter of 1866-67. The river at this part is navigable for ships of 2000 tons burthen, and is about 650 feet wide and 38 feet deep at high water of spring tides, the tidal range being 25 feet. The bed is composed of marl, or sandy clay, for a depth of 12 feet, followed by a stratum, 1 foot thick, of débris mixed with branches of trees, &c., succeeded by a bed, 14 feet thick, of softer marl, or sandy clay, beneath which is a stratum of débris as before 1 foot thick, followed by soft marl for 6 feet, and then from 6 feet to 7 feet of gravel and clean shingle overlaying the rock. From the bed of the river to the surface of the rock these deposits are as under:—

	Feet.
Hard marl	12
Débris	1
Soft marl	14
Débris	1
Soft marl	6
Gravel and shingle . . .	7
	41

The old wooden bridge was of a type very common in Ireland, and had been built about seventy years. It consisted of a series of trestles, of pine timber balks, bolted together. To the lower ends of the pine balks, forming the legs or piles of the trestles, logs of oak, about 5 feet long, were spliced. These trestles appear

[1] The discussion on this Paper was taken in conjunction with the two preceding ones, on somewhat similar subjects, by Mr. Grover and Mr. Conybeare, respectively, and occupied portions of two evenings.

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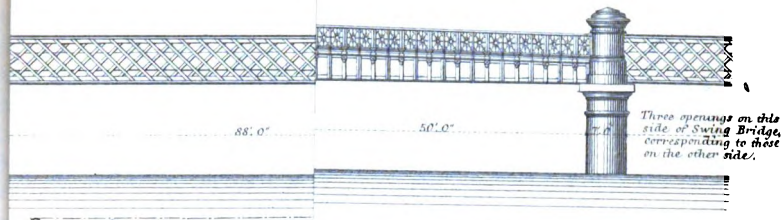


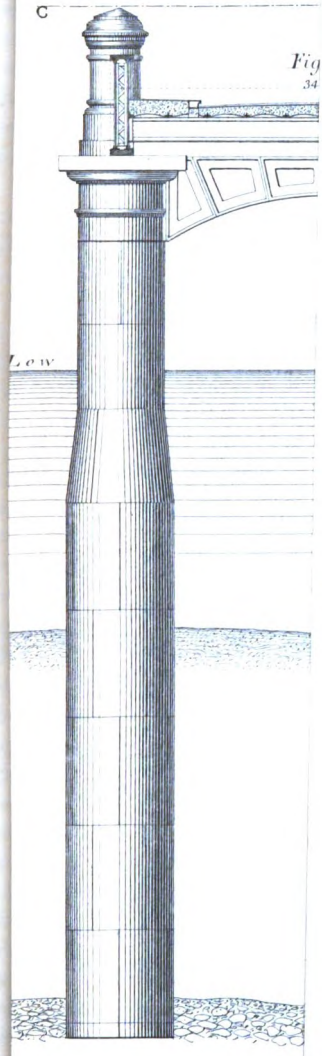
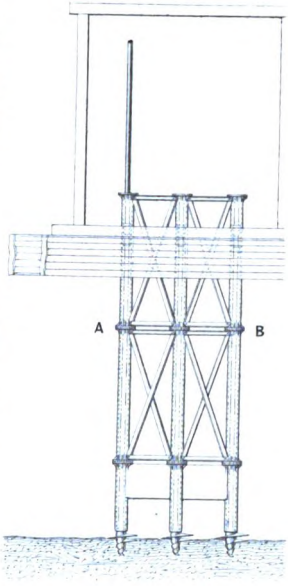


Fig: 7.



PIER AS PITCHED READY FOR SCREWING

Fig: 8.

SECTIONAL PLAN AT A.B.

KELL BROS LITHRS CASTLE ST HOLBORN

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to have been put together on shore, and to have been afterwards floated to where they were to be raised into the vertical position, and to have been then sunk about 3 feet into the bed of the river by weighting. The trestles were placed 25 feet apart, and longitudinal balks were laid upon them, with a covering of planking, 3 inches thick, and a wooden handrail at each side, giving a roadway 27 feet wide. A portcullis, or bascule, of about 30 feet opening, was provided, by framing together a few light balks of timber, and covering them with planking, so as to form a pair of 15-feet doors, which were hinged to the fixed work, and were capable of being lifted vertically, by a windlass and ropes placed on each side of the opening. This bridge is said to have cost about 10*s.* per square foot of surface.

The new bridge is chiefly of iron, and was designed by the county engineers, Mr. James B. Farrell, M. Inst. C.E., and Mr. P. Burtchaell. The superstructure consists of four fixed spans of lattice girders, each of 88 feet, and of a swing bridge of two spans of 50 feet each. The roadway is carried on the top of the swing bridge girders, and between the main girders of the fixed spans. It is 32 feet in width, including a footway on each side of 5 feet 6 inches, giving a clear carriage way of 21 feet. The total length, including approaches, is 700 feet. There is a rise of 3 feet in the roadway, the highest part being 10 feet, and the lowest part 7 feet, above high water of spring tides. An ornamental parapet, supported on cantilevers, with suitable lamps at the centre, surmounts the swing bridge, which is near the middle of the structure. The fixed spans consist of pairs of continuous wrought-iron lattice girders, 8 feet deep by 1 foot 10 inches wide, sufficiently long to reach over two spans; they are placed on the outside of each footway, and form the parapets. These girders are composed of plates and angle iron, with the usual trough-shaped section of flanges, having a sectional area of from 42 square inches to 62 square inches, the greatest section being at the middle of the girders over the points of support. To the flanges lattice webs of angle iron and flat bars are attached diagonally, crossing each other at angles of 45°, and at distances apart of 3 feet from centre to centre. Cross girders of plate and angle iron, 1 foot 6 inches deep and 3 feet apart, rest on, and are fastened to, the lower flanges of the main girders, and are covered with Mallet's buckled plates, 3 feet square and $\frac{3}{16}$ths of an inch thick, supported longitudinally by T iron and transversely by the cross girders. The buckled plates are covered with a layer of 6 inches of Portland cement concrete, mixed in the proportion of 1 of cement to 5 of

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fine gravel.[1] Above the concrete is a layer of asphalte, 2 inches thick, composed of 2 parts of gravel to 1 part of pounded iron slag, mixed and laid on hot with pitch and tar in the proportion of 2 of pitch to 1 of tar. Upon this limestone metalling, 4 inches thick, is laid to form the surface of the road, and a layer of fine gravel for the footpaths. A cast-iron gutter kerb, with suitable openings and outlets, extends the whole length of the bridge on each side of the roadway.

The piers for the fixed spans consist of cast-iron cylinders, bolted together in segments, and filled with Portland cement concrete, mixed in the proportion of 3 parts of gravel, 2 parts of sand, and 1 part of cement. The cylinders are placed in pairs, at distances of 34 feet apart from centre to centre, so as to be directly under the main girders, and they are connected to each other by arched bracing girders. The lower lengths of the cylinders are 9 feet in diameter, the metal being 1½ inch thick, while the upper lengths are 7 feet in diameter, and the metal 1 inch thick, there being a taper intermediate piece connecting them below low-water line. The top of each cylinder is finished with an ornamental cap and moulding. The central pier, on which the swing spans rest, consists of a cluster of five cylinders similar to the others, but having slightly different dimensions, the lower lengths being 8 feet and the upper lengths 6 feet in diameter. Four of these are placed at equidistant points around a circle 24 feet in diameter, while the fifth cylinder is placed in the centre. The space between the four outer cylinders in the circumference of the circle is enclosed to below low-water mark by segmental plates of cast iron, and in the enclosed space a system of stay-bracing connects all the outer cylinders and the segmental plates to the centre cylinder.

A strong circular cast-iron girder, 24 feet in diameter, cast in segments and planed on the upper surface and bearings, rests on the four outer cylinders and connecting segmental plates; the upper planed surface forms a roller-path upon which twenty cast-iron rollers, having radial arms fixed to a live ring, revolve, carrying a similar upper roller-path upon which the swing bridge rests, and the bridge is worked by a chain and gearing from above in the usual manner.

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[1] A block of this concrete, 3 feet square and 6 inches thick, was moulded on the top of a buckled plate, and left until dry. The plate was then removed, and the block was tested by resting it on two bearings 2 feet 1 inch between the supports, two of the sides being unsupported, when it broke with a weight of 7 tons placed on it near the centre.

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The abutments of the bridge were to have been of stone, but, at the Author's suggestion, they were made of iron, partly because there was so little time for the completion of the work, and partly on account of the difficulty of making the necessary cofferdams, which would have interfered with adjoining property. The abutment on the Wexford side is composed of three cast-iron cylinders 6 feet in diameter, sunk at the face of the old wall to a depth of about 60 feet to the rock, and filled with concrete of the same quality as that used for the piers. Between the cylinders there are segmental retaining plates, sunk deeply into the ground by weighting, their concave sides being turned towards the river. Wing plates were also attached on either side, to give an appropriate form to the abutment, and were also sunk into the hard ground below low-water level. The space behind the retaining plates and the wing plates, and between these and the old wall, is filled with concrete and rough masonry up to the level of the girder beds, and the whole is surmounted with ornamental, fine-cut granite. The abutment on the Kilkenny side was constructed in a similar manner, the only difference being a slight variation in the form of the wing plates. These cast-iron abutments were found to be less costly than stone ones would have been, and enabled the erection of the superstructure to be proceeded with immediately on a sufficient number of cylinders being sunk, and they probably saved many months in the completion of the work—a matter of importance to the inhabitants of the locality, who had been deprived of the only bridge crossing the river within a distance of many miles.

Considerable difficulty was experienced at the commencement of sinking the cylinders from leakage, though it was at first hoped that sufficient clay would have been found to hold back the water. After building up a cylinder 45 feet high, it was lowered to the bed of the river, and was found to reach sufficiently above high water for the attachment of another length. The interior was then excavated by the sand pump, until the cylinder had sunk 14 feet, when an additional length was added, and weighted as before. After remaining a few hours, it suddenly sank 13 feet, carrying away some of the braces of the staging, which was not arranged for so great a movement. The damage to the stage was repaired, and the water was pumped out of the cylinder, but the inside could not be kept dry; for, as soon as the earth at the bottom was disturbed, the workmen were driven out by water coming up through it. Compressed air apparatus was then used, and the earth excavated to the lower edge of the cylinders. On the

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pressure of the air being removed, the cylinder immediately went down 13 feet more; and, on examination, there was found a depth of 20 feet of earth inside. When the earth was taken out, good hard gravel was found, into which the cylinder had penetrated 5 feet; and, as this gravel overlay the rock, it was deemed a sufficiently good foundation. In the course of sinking the cylinders various means were tried to pump out the earth from inside the cylinders, but it was found too tenacious to be removed satisfactorily in this manner. Mr. Milroy's excavating apparatus was also used, and acted very well, but it was eventually found necessary to complete the work by compressed air apparatus. All the cylinders in the bed of the river were liable to descend suddenly from 6 feet to 13 feet, when they reached a certain stratum; after the first was sunk, suitable arrangements were made in the staging to meet this.

The time occupied in sinking a pair of cylinders to the proper depth was about ten weeks. On one pair operations were commenced on the 3rd of December with the sand pump and Mr. Milroy's excavator, which were replaced on the 5th of February by the compressed air apparatus, and the pier was completed fourteen days afterwards, at a cost of £250 for labour, exclusive of the staging and tools. The costly nature of the operations required for sinking cylinders in deep water, and the time occupied in fixing them, led the Author, during the progress of this work, to design an open braced structure for such cases, the pier being made to carry the same superstructure as that just described. This pier is composed of a set of braced tubes, with screws running through them; the braced structure, or set of four screws with tubes, &c., after being built, is lowered through the water; and, on reaching the bed of the river, sinks as far as its weight will carry it; the screws are then made to revolve, being held in position by the tubular braced structure, and by the ordinary means are driven as far as necessary. They are afterwards cut off even with the joint of the tubes, and a bearing plate inserted, upon which the superstructure is carried. Such a substructure in the bridge now described would have saved £8,000 in the cost, and could have been built in much less time, besides offering less obstruction to the waterway.

The disposition of materials in the bridge are as follow:—

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	Tons. cwt.	Tons. cwt.
Cast iron in abutments	180 12	
,, in piers	966 17	
Bolts, &c.	34 19	
Total ironwork in piers and abutments		1,182 8
Swing-bridge ironwork girders, &c.	143 7	
,, handrail	15 7	
Fixed spans, girders, &c.	434 18	
Machinery, rollers, &c.	56 15	
Total of ironwork in superstructure		650 7
		1,832 15

Masonry in abutments and approaches	10,656 cubic feet.
Cement concrete	2,000 cubic yards.
Timber in swing-bridge floor	3,000 cubic feet.
Elm planking	120 ,,
Timber in dolphins	9,750 ,,
Metalling	227 cubic yards.
Asphalte	1,360 square yards.

The actual cost of sinking the cylinders was—

	£. s. d.	
Cost of staging, that is, labour and the use and waste of materials in the same	0 15 0	per ton of iron fixed.
Fitting together, erecting in place, and sinking, including excavating the earth from inside, partly under compressed air and partly by other means	4 2 6	per ton.
The difference in the cost of tools used in fixing cylinders, and the value of the same when the work was completed, including air-pumps, engines, &c.	1 17 6	,,
Cost of superintendence and sundries	0 14 0	,,
Cost of cement-concrete filling inside the cylinders	1 1 0	per cubic yard.

The work for the bridge was executed by the Messrs. Kennard Brothers, under the Author's superintendence, and was commenced in April, 1868, the roadway being opened for public traffic in July, 1869. The total cost was £36,250, or about £2 5*s.* per square foot.

The communication is accompanied by a series of drawings, from which Plate 12 has been compiled.

[Mr. J. W. GROVER

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Mr. J. W. GROVER observed in respect of the head of the Clevedon pier, that considering its horizontal dimensions, 50 feet by 40 feet, it might seem extraordinary that a structure of this kind should have been built on so small a ground-plan. It was not the original intention to have constructed so small a head; but it was consequent on the want of funds to do more. The height, 70 feet, was out of proportion to the width; and in consequence, the head at low tide, from the sea, appeared little more than a tower, and as such, had to be strongly braced, to resist the great force of the impinging tide.

The general idea of a pier was a structure running through water of moderate depth, and a rise of tide of 15 feet or 20 feet at the most; but with a tower 70 feet high in a depth of water of 52 feet, it became a question how to resist the shocks of steamers; and, at the same time, to prevent the structure from offering an undue resistance to the passing volume of tidal water, and to the waves of translation and oscillation to which it was exposed. The fender-piles, therefore, were only 8 inches square, and though that was too large as far as the waves were concerned, it was too small with regard to the vessels. He never intended that the head should be left unprotected, to sustain the shock of sea-going steamers of 500 tons to 600 tons; but had recommended that a lighter should be stationed outside, as at New Passage. This advice had not been followed, and the consequence was the fender piles were constantly being carried away, or the shock from ships soon reduced the front piles to the condition of splinters. Larger ones had been substituted, which had resisted the effects of the steamers; but had the defect of holding the water, and thus bringing an undue strain upon the structure. He thought fenders might be made advantageously of iron where it was desirable to avoid the action of the sea, with spiral springs to act as buffers.

For structures in sea-water more reliance was in his opinion to be placed upon wrought iron than upon cast iron. Although wrought iron rusted rapidly, yet the intensity of the action could be measured, whereas the decay of cast iron was uncertain. He had found cast iron exposed to the sea entirely disintegrated by the action of the water. When the cast-iron skin was unbroken as it left the sand, the coating of silica acted as a natural protection; but in parts where the iron underwent any considerable manipulation, by being turned on a lathe, or being perforated by drilling, that protection ceased to exist. However, the stability of the structure depended upon the wrought-iron ties, which formed the element of its greatest strength.

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With regard to this work as a piece of construction, he would draw attention to the employment of arches below as lower members of the girders. The strains on these were proportioned to their depth and angle of thrust. Moreover, it would be possible to saw any girder through at the middle of the span, and yet as cantilevers the halves would stand. He alluded to that because he had heard so many discussions on the question of employing arches as girders, combined with horizontal members, and obtaining the proper strain upon each. He thought it desirable in every case to consider the question as if the girder was intersected at the centre, and that the half-span formed a jib.

The deflection upon each span varied from 1 inch to 1⅜ inch, the line representing it passing through the structure in a series of waves; and it was greatest at the two extreme ends. The girders were continuous: there was no break at any point, and at no part were they exposed to a strain greater than 4·3 tons per square inch; and that occurred in the fourth span from the abutment.

Mr. ABERNETHY remarked that there was nothing peculiar about the Clevedon pier; many of a similar character had been erected at different parts of the coast. There appeared, however, to be something original in the mode adopted in getting in the foundations of the Cambrian railway viaducts. He understood that at one spot there was a considerable current through that bridge; and that being the case, he conceived that the pier might subside, inasmuch as the mass of stone placed on the sand would be liable to be undermined by the strong tidal flow.

Mr. H. CONYBEARE said that the stones thrown down over the sandy part of the channel had the effect of fixing the sand. The stone was extended beyond the pier, and there had been no signs of subsidence.

Mr. ABERNETHY conceived that the stone should be carried across the channel, and even then he should be apprehensive that subsidence would take place. He thought it would have been better to have dredged out the sand, down to the gravel, and then to have deposited the stone.

Mr. CONYBEARE replied that any sand that might have been dredged out would probably have been replaced the next tide. When the current was rapid, the water looked like pea-soup, from the great quantity of sand in it.

Mr. ABERNETHY said he should have preferred sinking a caisson through the sand; it could then have been filled with stone, and the discs placed upon the top.

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Mr. HEMANS, Vice-President, observed that an opening bridge on a railway was, in his opinion, the greatest nuisance an Engineer could encounter. He had built ten or fifteen such bridges across rivers and estuaries; a great proportion of which were wholly unnecessary. There appeared to be at all times a species of mania on the part of those who commanded often the most paltry navigations to demand opening-bridges, which were, in many cases, really of no use. Some that he had built of from 40 feet to 60 feet span had never been opened since the day they were put up, although a great deal both of time and money was expended upon their erection.

The drawbridges on the Cambrian railway had been ingeniously carried out. A similar sliding-bridge he had seen at Rhyl worked exceedingly well; but it was an error to suppose that there were only the two kinds of telescope-bridges mentioned, viz., one rolling over and one rolling under, in addition to the swivel-bridge. There was a third kind, of which he had built specimens over the Severn[1] and the Shannon; and these consisted of a sliding-bridge which did not alter its level; but room was made for it to slide back into a recess behind.

The extreme end of the arm of a swing-bridge moved the length of a quadrant, whereas a sliding-bridge moved only the length of the radius; whatever the weight was, the latter had to be moved two-thirds of the length of the swing-bridge. He was therefore decidedly in favour of the sliding-bridge wherever it could be adopted for moderate spans; besides, it kept the navigation more clear than a swing-bridge with projecting arms, which, unless protected at great expense, as in the bridge over the Ouse, on the North Eastern railway, were likely to interfere with the rigging of vessels in a cross wind.

Mr. REDMAN remarked, that to have erected a landing pier, in an exposed position like Clevedon, 800 feet long and 70 feet high, for the sum of £10,000 was no ordinary achievement. As usual the chief proportion of the outlay and difficulty was incurred on the head of the pier.

There was one question on which he could not help joining issue with Mr. Grover—and that was with reference to the durability of cast iron for works of this nature. He thought the cases referred to, of the rapid deterioration of cast iron, must have arisen from some peculiarity not described, or from the quality of the iron itself. He could refer to works of cast iron on the coast which had been

[1] *Vide* "The Engineer," May 7, 1869.

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erected thirty or forty years, and which he knew personally to be perfectly sound and intact.

He considered that the pier at the Barmouth viaduct must depend for its stability upon the series of lower screw-discs used there; and therefore, in all probability, if there were movement of the sand the upper discs would be found rather an encumbrance to the structure than otherwise. The Trinity House authorities, about thirty-five years back, put down cast-iron screws in a rapid tide-way at sea, for the Maplin sand lighthouse, which was built of an octagonal form upon Mitchell's screw piles;[1] and it was there and at the Fleetwood lighthouse that the first application of those piles was made. Some hesitation was felt in adopting that mode of foundation, and to make sure, after the screws had been worked down into the sand, a grating of timber was formed outside, and was floated over the screw-piles. The grating was then sunk by loading it with Kentish rag-stone; but, though intended to protect the lighthouse, it was nevertheless an incubus, for the sand had subsided considerably and the caisson had been rather a dead weight than an assistance. When first placed it was dry at low water, but it was now in a considerable depth of water at that period of the tide.

With regard to the question of the telescope-bridge, in one of the early metropolitan dock works there was a small foot-bridge called Palmer's bridge, which he had been in the habit of walking over, that had precisely the movement described by Mr. Hemans; it was drawn backwards and forwards in a similar manner and was housed below the quay level.

Mr. G. B. Bruce inquired what was the weight of the monkey with which the wooden piles were driven in the case of the Cambrian railway viaducts. He understood that a piece of lead was placed between the top of the cast-iron socket and the monkey; but he could not imagine how a pile under such circumstances could be driven at all, for he thought the monkey would soon crush the lead and knock the cast iron to pieces. He always drove the pile first, then cut it off and afterwards bolted the socket on the top of it.

With regard to the stone-work round the pier, he had no doubt that the stone would sink through the sand, and that the large stones would be found resting upon the gravel in a conical form, and so would afford some strength and protection to the piles.

Mr. G. H. Phipps referred to a drawbridge of somewhat peculiar

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. ix., p. 193.

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construction, and the only one upon the London and North Western railway. This bridge, on a plan proposed by Mr. Stephenson, was erected under his directions to carry the railway over a narrow branch canal leading out of the Grand Junction canal at Weedon, in Northamptonshire, into the Government depôt. The peculiarity of that drawbridge was that when required to be opened it was moved in a direction at an angle of 45° with the line of railway, by means of three wheels running upon two lines of rails laid on the masonry beneath the platform of the bridge, and working into racks to keep the motion square on. There was a sufficiency of the platform beyond the side of the canal to ensure the centre of gravity of the bridge being at all times within the triangular space formed by the three wheels, and so to counteract any tendency to tilt over.[1]

Mr. R. P. Brereton observed that the piers at New Passage, which he had erected about 13 miles higher up the Severn, extended farther out from the shore than the Clevedon pier; but the difficulties experienced were very similar, there having been a still greater velocity of current, and a rise of tide of 50 feet; and some of the piles were 85 feet long, and stood 70 feet above the ground. The piers with the construction of which he had been connected were substantial timber structures with spans of 22 feet opening, capable of withstanding rough usage and heavily loaded to resist the sea; and built for the purpose of steamboat and railway traffic, without the light and ornamented appearance which was desirable in the case of a promenade at a fashionable watering-place. The cost of the Clevedon pier, 16 feet wide and 76 feet in greatest height, appeared to be about £40 per lineal yard, which was nearly the same rate as the other piers, but the width being for a double line of rails, about 30 feet, or 50 per cent. wider than at Clevedon, surface for surface, those structures seemed to have been cheaper.

It had been remarked by Mr. Grover, that the most economical span to adopt would be when the cost of the pier was the same as that of the superstructure. This was not necessarily the case and must not be taken as an axiom, as the total cost of a structure might be least when the respective cost of the piers and of the superstructure were very different. It depended materially on the height and number of the piers. Theoretically, the cheapest structure would be where there were no openings and everything was in the piers; on the other hand, a very deep ravine might be crossed advantageously by

[1] *Vide* Papers on subjects connected with the duties of the Corps of Royal Engineers, vol. iii., p. 189.

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a bridge without piers, the whole cost being in the superstructure. In an iron structure where the supports could be used to their full advantage without having to fortify them to prevent flexure, as for instance in the rods carrying the floor of a suspension bridge, whatever the height might be, the minimum of material would be found when the suspension rods were close together without any girders between to form the superstructure.

As to the relative durability of wrought iron and cast iron, there were numbers of cast-iron structures in sea-water which had stood for thirty or forty years without symptoms of decay. With regard to wrought iron, no doubt small wrought-iron rods did stand the sea and the effects of salt water very well. He had occasion to use a number of them ten years ago for marking out a line of training bank on the coast of South Wales, and they had not corroded in the least. These rods were about 1¼ inch in diameter, standing 10 or 12 feet out of the sand; they extended 2 miles from the shore into the sea, and were frequently subjected to heavy breakers. It seemed that a film or fur, resembling galvanized iron in appearance, formed upon the iron when it was round, but when there were square edges or arrisses to be chafed off, a polished surface, from abrasion of the metal, continually recurred, and fresh coatings of rust were formed, and in time the bars would perish. The commonly-received explanation of the fact that iron articles in salt-water mud remained uncorroded, while they speedily perished in sea-water, was that the iron took from the salt water a certain amount of fixed air which it contained to form rust, but after this the water became comparatively harmless, and when the film of rust was undisturbed the iron remained in its original condition; but where the water was renewed, and the rust got rubbed off, oxidation went on and the article was at length destroyed. In the holds of iron ships there were places where small quantities of bilge water sometimes remained on the plates for months together without increase of corrosion taking place; whereas in other cases, from the action of sulphur and ashes from the furnaces, rivet heads and patches of the plates would be eaten out and the plates would be rapidly destroyed. He did not think there was any peculiarity in the salt water which was particularly damaging to wrought or cast iron; the mischief arose from the surfaces exposed to chafing and the constant renewal of the water.

The different varieties of drawbridge that were to be met with on the South Wales railway did not appear to have been noticed by Mr. Conybeare. Between Gloucester and Milford Haven there were seven opening bridges, of which six had been regularly

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at work for the last twenty years without any accident; and in one instance, as Mr. Hemans had observed, a drawbridge had been put up but had never been opened. Out of these seven bridges only two were of the same description, owing principally to the different circumstances under which they were erected. Some of them had clear openings of 50 feet, and, being skew, required 70 feet of overhanging leaf.

The bridge described by Mr. Conybeare was apparently a modification, and in one respect rather an improvement, upon the rolling bridge across the river Towey, at Carmarthen, erected in 1852; but it had the disadvantage of requiring a separate bridge to carry its weight when rolled back. There were several kinds of opening bridges, including the bascule, the hinge or lifting, the swing or swivel, and the hydraulic lifting pivot description; but the Carmarthen bridge was the only one of the rolling kind at the time that he was acquainted with. It was not telescopic, but the moving part extended over two entire openings and was rolled backwards above the floor of the adjoining viaduct.

He had first suggested a drawbridge of this kind, in 1850, to cross the Rhine at Cologne, where there was to be an opening of 96 feet, and with no facilities for getting in a large centre pier and fenders for the protection of a swivel bridge. The plan was ultimately adopted by Mr. Brunel at the Carmarthen bridge. Mr. Brunel had been much in favour of the hinge system of lifting bridges, which could be raised for opening and lowered upon dead points or solid bearings free from the trembling tendency of the rollers of a swivel bridge under the weights of some hundreds of tons of passing load besides the weight of the bridge itself; but being skew, and with a span between 50 and 60 feet, the raising in the air of so large a surface of double line bridge would have been a formidable matter. The bridge, as constructed, had no running wheels or rollers, and resembled some that had subsequently been put up at the Swansea docks. The piers required were narrow, occupying but little water-way, and had fixed wheels upon the tops of them; the rails were attached to the underside of the girders, which, when closed, rested upon dead points, and when opened were tilted about 1 foot to clear the rails and then rolled back; and there was no necessity for any under structure to carry the roller. He did not think there had been an earlier instance of that construction than the Carmarthen bridge, built in 1852.

Mr. ABERNETHY remarked that in one situation it might be desirable to construct a swivel-bridge, in another a telescope-bridge, and in a third a hoist-bridge. Mr. Hemans stated that he pre-

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ferred the draw-bridge to the swivel-bridge; but, when practicable, he preferred the latter, an excellent example of which had been constructed by Mr. Harrison over the river Ouse, near Goole.

It was impossible to adopt swivel-bridges at Swansea, because the width of the river Towey was only 205 feet, and sufficient water-way had to be given for vessels to pass up and down the river, and the pier necessary for a double swivel-bridge would have occupied too much of the water-way. Moreover, there was the danger of vessels running up the river under canvas coming into collision with the open bridge, and that danger could only be avoided by giving considerable additional width to the pier; he therefore divided the space into three spans, two of 60 feet and one of 61 feet. The opening portion of the bridge crossed two of these spans, and passed over fixed rollers on the pier and west abutment. When closed, the bridge formed a gradient of 1 in 56 with the load. In opening the bridge, the end resting on the second pier was released, which caused the bridge to tilt on the centre rollers, raising the end at the abutment sufficiently high to pass over the fixed rollers, and it was then drawn back by hydraulic power.

The bridge across the docks under other circumstances differed materially from that over the river. To avoid interference with the gearing of the lock gates, this bridge, instead of passing over fixed rollers, rolled back upon a roller path, independent of the lines of rail. It was lifted in the centre by a hydraulic ram; at the end of the arch girders there were horns which locked into three slots in the abutment. When the bridge was raised the horns bore upon rollers at the top of the slot, and that brought the end of the bridge sufficiently high to rest upon the roller path, when it was also drawn back by hydraulic power. The weight of this part of the bridge was 200 tons, and of that over the new cut 205 tons: they had been at work since 1864, and were, on the whole, a success; but he should in all cases, where it was practicable, adopt the swivel-bridge, in preference to the draw-bridge, as being worked more easily, having much less friction, and capable of being opened and shut in much less time.

He thought the profession at large was greatly indebted to the firm of Sir W. Armstrong and Co. for the high perfection to which they had brought hydraulic machines, without which it would have been impossible to work bridges of this magnitude in such a short period of time.

Mr. HEMANS, Vice-President, observed that he had visited the New Ross bridge, and could bear testimony that it had been

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carried out under serious difficulties. There was not a more important road and navigation bridge in Ireland at a junction between two counties, and he regarded it as one of the most perfect of its kind.

Mr. W. B. LEWIS asked Mr. Maynard the cost per cubic yard of the cast-iron abutment filled in with concrete, so that it might be compared with that of masonry? also the nature and the proportionate quantities of the materials used in the concrete with which the cylinders were filled, and which was said to have cost 21*s.* per cubic yard.

Mr. BRAMWELL enquired whether the alternative plan of a pier composed of four hollow piles, each having a screw pile within, was more or less expensive than that which was actually adopted, and whether Mr. Maynard knew of any instance in which the plan of the hollow pile with the screw pile within had been executed?

Mr. W. LLOYD questioned whether it was always judicious to adopt screw piles in preference to more solid constructions. It was difficult to keep screw piles secure, and he thought, in sinking the piers of the New Ross bridge, greater difficulties had been encountered at some parts of the foundation than at others. In such a case there was the same chance of failure as he once encountered in a river bed in South America, which in the time of flood was a quicksand. The piles were screwed down to what he thought was a perfectly secure depth; in fact, till they were twisted almost in the shape of a corkscrew. This was in the dry season; but when the rainy season had set in, with 20 feet of water on the sand, it was found that the piles were standing without any foundation, and eventually the pier was swept bodily away. He therefore was averse to using screw piles in preference to the solid pier adopted at New Ross, which he was certain was the safest that could be constructed.

Mr. H. N. MAYNARD stated that since the New Ross bridge was erected, experiments had been carried out to test the sleeve bracing he then proposed, and a great many piers, or rather abutments, had been made on the same principle with satisfactory results. He had now on hand a large number of road bridges, and they were all made upon this principle. The abutments were composed of a number of cast-iron columns or sleeves braced together with plates and angle-iron entirely filling up the spaces between the piles. They were placed in position with wrought-iron screws of sufficient length to reach through the top of the sleeves; and the capstan was fixed on and the screws driven to the required depth. The set of

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four columns were lowered into the river, and by the capstan the piles were driven down to the hard ground. The number of columns varied, according to circumstances, from three to five or seven. There were four in this case, as giving the proper sectional area to the pier. In some instances this plating was carried all the way up. It was weighted down and the material dredged out from the inside, and after the whole was sunk as far as the dredging would allow, the screws were driven to the requisite depth. The abutments he had now in hand, for the most part, weighed 9 tons, and were about 14 feet in height for roadways that were 18 feet wide. The quantity of iron, and consequently the cost, depended upon the width of the roadway and the height of the abutment. He could not, therefore, give any direct comparison as to the relative cost between an abutment upon this principle and one of masonry; but in a country where masonry was expensive, the facilities for erecting this kind of iron abutment were so great as to counterbalance any trifling difference of cost. Abutments of iron on this principle might in some countries cost a little more than abutments of masonry, but against that must be set the rapidity of execution and the facility of transporting the materials ready to be put together. The people who erected the iron portion of the New Ross bridge were capable of erecting the iron abutments at the same time, and making the work perfect as they went on without the employment of masons.

This sleeve-braced structure might be built to a considerable height, and the braced cluster lowered through a depth of 150 or 200 feet of water, and the screwing process carried on by a capstan above, the structure forming its own staging. It would be found very inconvenient, perhaps impracticable, to sink cylinders through so great a depth; but there was no practical difficulty in carrying out a pier of this kind through that depth of water, and it was intended for such conditions. The braced cluster weighed less than 5 cwt. per foot in height as compared with a cast-iron cylinder of equal strength at 14 cwt. per foot in height. The abutment rested not upon the flanges of the sleeves, but upon the screw blades, and the bracing would be put upon them at any depth, so as to make the whole cluster act the same as one pile of that size. The bracing served as a temporary stage while the screwing in was being done, as well as for a permanent bracing; and the screws might be driven as far as required through the bracing. The casing or sleeves and bracing took the place of the staging while the screws were being driven; if the bracing was attached to the screw the screw must stop where the bracing held it. The

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cylinders of the New Ross bridge were filled with Portland cement concrete, at a cost of about £1 1*s.* per cubic yard. The structure might be wall-plated instead of being of open bracing, and filled with concrete, and in some instances he had arranged for the plating to be carried up to the top. Every piece of the abutment wall was of a weight easy to be transported to inaccessible parts of the country, and to be fixed with bolts and nuts. In one case he had one hundred abutments to send out for distribution over many miles of country, where there were at present no roads, the bridges having to be made first and the roads afterwards. The locality was one where masonry and building materials of all kinds were very difficult to obtain, and these abutments were speially adapted to a case of that kind.

Mr. W. SHELFORD said that fourteen years ago he was engaged under Mr. Fowler in the completion of some swing-bridges over the river Nene. One of these bridges, on the pivot principle, measured 104 feet from the centre to the longer end, and 52 feet from the pivot to the extremity of the short end; the difference in length being compensated by a weight placed in a balance box. The width between the girders was 28 feet, and the weight 500 tons. It was worked by the Armstrong hydraulic accumulator; the accumulator weighed 50 tons, and the area of the ram under the bridge was $\frac{1}{10}$th of that in the accumulator. There was nothing new about the construction, but it occurred to him to point out the great importance of time in the working of these bridges, an importance which increased in proportion to the traffic over the bridge and the consequent weight to be moved. The bridge he referred to was a great success in that respect. It could be swung over the river in two minutes, and the time would have been less if there had been sufficient power at command; but it required from four to six men to pump for at least an hour in order to get this 50-ton accumulator to a sufficient height to lift the bridge. It was originally intended to have a steam-engine to lift the accumulator in a few minutes to the required height, but that was abandoned. In point of fact the bridge was built by order of the Admiralty at other people's expense, and was never used simply because it was never wanted.

There was another bridge about 5 miles below Peterborough of a similar calibre to that described by Mr. Conybeare. It was a swing-bridge worked by a rack and pinion, and the only occasion on which that was opened was under peculiar circumstances. It was not sufficiently secured to its bearings; a gale sprung up one night and the next morning the bridge was found blown over the river.

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That was not the first time that he had known such bridges influenced by wind. A bridge which in calm weather could be worked by one man could not be worked at all by him in a wind. He thought Mr. Conybeare's bridge was free from this objection, but was inapplicable to large spans and weights of 500 tons.

Mr. BRUNLEES said that Mr. Piercy, while the Engineer of the Cambrian railways, had designed the viaduct for the crossing of the estuary at Barmouth, yet his name had not been mentioned by Mr. Conybeare, either in the Paper or in the course of the discussion. He had with him the whole of Mr. Piercy's drawings for the viaduct, and they differed very slightly from those described by Mr. Conybeare, who said that he had designed the discs represented, and had distributed the bearing on the piles by means of the number introduced.

When Mr. Piercy was about to design the viaduct in question, Mr. Brunlees lent him the drawings of several viaducts for crossing similar estuaries, and also advised him as to the best method of getting over the difficulty arising from the stratum of peat that had been referred to. It would be seen, from an inspection of the drawings, that the piles were designed with a disc, and with the same description of screw at the bottom as was shown by Mr. Conybeare, and were in fact identical with those carried out. He merely wished that Mr. Piercy should have whatever credit was due to him, and he hoped that Mr. Conybeare would award it to him.

As regarded the stoning of this viaduct, it was out of the question to suppose that the stone would lie upon fine sand such as was found in the estuary at Barmouth. When the rubble was placed in such a position it presented an obstruction to the tide, which washed away the sand below, and the stone settled down and occupied its place. During the last year he had completed a viaduct across the Solway Frith, nearly 1¼ mile in length, and the stoning was executed in the following manner:—Openings were left at intervals in the superstructure of the viaduct, and through these the stone was 'tipped' in heaps. As soon as the tides acted upon the sand, from the obstruction presented by the stone, it was washed away and the stone took its place. In this manner the whole of the bed of the estuary, in the line of the viaduct, was covered with a platform of stone, to a depth of from 4 to 8 feet, giving great support to the piles. He might mention as a proof of the solidity with which stone so disposed held its place, that the Morecambe Bay viaducts were originally constructed for a single line of railway, but the stone was put in for a double line.

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When the viaducts were widened, large bearing stones were bedded in the rubble and served for the foundations of the columns. The viaducts were thus doubled at a comparatively small cost, and the work had stood without any settlement whatever, showing conclusively how close and compact the stones became after they were once laid down.

Mr. J. W. GROVER had only one observation to make, and that was as to the question of cost, as compared with the piers to which Mr. Brereton had alluded. He found that the actual cost of the body of the Clevedon pier was £6,800, and that divided by 800 feet gave £8 10*s.* per lineal foot, and consequently £25 10*s.* per yard. He understood Mr. Brereton to state that the New Passage piers cost only £40 per yard for a width of 30 feet; but it was necessary to bear in mind that those piers were of timber, with 20-feet openings only, while the structure he had described had the large openings of 100 feet, and was designed to carry a moving load of 16 cwt. to the lineal foot. The actual test weight placed on the centre of one span was 42 tons, a load approaching that induced by ordinary railway traffic. The difference, if any, in cost might be accounted for simply by the fact of iron having been employed, and he need not say that timber was much cheaper than iron in first cost; but notwithstanding that, the New Passage pier seemed to have cost as much per foot, though built in a river, as the Clevedon pier, which was placed in an open sea-way, and exposed to severe gales from the Atlantic.

Mr. VIGNOLES, President, observed that two or three centuries ago, during the great wars in the Netherlands and in Spain, amongst the physical difficulties that were encountered there was particularly that with respect to diversion of streams of water, and obstruction of the passage of troops and artillery across marshy grounds and occasional deep channels. To obviate these difficulties a system was invented, which was still carried out in the north of Europe, of forming fascines composed of osiers, technically named 'matrasses,' of considerable extent, loading them with stones, and sinking them. That plan was carried out for the protection of the low banks of the Elbe, even down to near its mouth, where for a number of years they had constantly required protection against the tides and sea. In the Zuyder Zee this practice was such as to form a remarkable feature there. The original intention was that they should lie nearly flat, while the loading with stones kept them in position; but the action of the water was such that in many cases they gradually sloped down to angles of from 10° to 45°, and at the Helder this had been the case

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to the extent of as much as 60° to 70°. The Danish engineers in the low grounds on the shores of the Baltic used this system of fortification for banks and foundations to a considerable extent. He had used it himself on a large scale in Russia and in Spain, and had always found it constituted a solid foundation for masonry, and, though the 'matrasses' subsided downwards outside and around the foundation platform to various angles, they always made a good base and perfect protection in depths of water of from 40 feet to 50 feet, and never stirred. With these layers of osier 'matrasses' laden with stones, and sunk around the masonry, whether executed in the shape of an apron across the stream or of piers, the work remained secure, even though the scouring out of the river bed brought them from their original horizontal position to a considerable depression, whether with the stream or otherwise. He believed this was a system which was scarcely known in Britain; but he had found, in dealing with sandy and soft foundations, that it was one of the most perfect kinds of foundation that could be adopted.

Mr. J. R. Mosse remarked, that the large navigable rivers which had to be crossed by railways in the United States obliged American engineers to adopt swing-bridges of timber of considerable spans. Several double timber swing-bridges of 120 feet span each, having a turn-table in the centre, had been constructed with complete success; and Mr. McCallum had designed a timber swing-bridge having two openings of 150 feet each, and resting upon a turn-table 29 feet in diameter, for carrying a railway over the Mississippi river at Clinton, in Ohio.[1] To the best of his knowledge, swing-bridges were more common in the United States than opening bridges of any other description.

Mr. H. Conybeare, in reply to the discussion on his Paper, observed that the foundation of the Aberdovey viaduct consisted of ordinary timber bearing-piles, to the top of each of which, as soon as it was driven down to the level of low-water, a cast-iron splice was affixed and driven home with it until the whole of the timber was sunk below the bed of the channel. The piles of the Dovey viaduct were found to drive quite similarly to the trial piles that had been previously driven as a basis for calculation. The timber piles were driven down to the level of low-water, at which level the cast-iron splices were attached to them by a 21-cwt. monkey, falling 8 feet; the last blow sinking them about ·7 of an inch. The fall and weight required for driving them home, with the

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxii., p. 315.

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cast-iron splices affixed, below this level, so as to give them the requisite supporting power, were then calculated by the formula of Major Sanders of the United States Engineers, which was based on experiments tried at Fort Delaware, to determine the supporting power of piles, in the year 1849. By this formula $W = \frac{\omega H}{8 H'}$, where W = weight that may be safely placed on the pile; ω the weight of the ram, in tons; H the fall of the ram at the last stroke, in inches; and H′ the distance moved by the pile at the last stroke, in inches. The piles, with the cast-iron splices affixed, were driven home with a 22-cwt. monkey, falling 3 feet; and, for the last two strokes, sunk ·3 of an inch per stroke. This gave, by Major Sanders' formula, 16 tons as the load that might safely be placed on each pile, or 112 tons as the load that might safely be placed on each pier of seven piles. The cast-iron splices were accordingly designed of sufficient strength to bear, without injury, the final stroke required by the formula in this particular case, and they did so. Of course, had a greater supporting power per pile been necessary, and consequently greater stress in driving, the cast-iron splices would have been designed accordingly; for, with proper precautions, it was obvious that an adequately designed cast-iron pile might be driven as hard as a timber one.

The extraordinary divergence of the various formulæ for determining the supporting power of piles was unparalleled in any other branch of engineering.

In the course of the discussion on Mr. McAlpine's Paper on the supporting power of piles,[1] Mr. A. Beazeley exhibited a table of the supporting power of the piles of the Cudgegong bridge, as estimated by various formulæ, showing that the supporting power of each of these piles, as calculated by Rankine's formula, was 5·20 times, and by Weisbach's 5·24 times, that given by Sanders' formula; and that was only $\frac{1}{5}$th the full measure of the divergence of such formula; for Mr. Bruce, in his account of the building of the Royal Border viaduct,[2] appeared to have used a formula that required, in order to obtain a supporting power of 75 tons per pile, a weight of 15 cwt. falling 16 feet, till each pile only moved $\frac{1}{20}$th of an inch for the last fall. Now, this gave by Sanders' formula

$$W = \frac{\cdot 75 \times 16 \times 12}{8 \times \frac{1}{20}} = 360 \text{ tons,}$$

instead of 75 tons; thus the formula that appeared to have been employed by Mr. Bruce in this

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxvii., p. 307.

[2] *Vide* Ibid., vol. x., p. 222.

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case gave a supporting power of scarcely more than $\frac{1}{2}$th of Sanders' formula, and Sanders' formula gave only about $\frac{1}{3}$th of Rankin's and Weisbach's; therefore the two latter would give twenty-five times as much supporting power as the formula that appeared to have been employed by Mr. Bruce with reference to the piles of the Royal Border viaduct. He quite concurred in the opinion expressed by Mr. Beazeley, that for practical purposes there was not a better formula than that of Major Sanders of the United States Engineers.

In the particular case of the Dovey viaduct a timber dolly was interpolated between the monkey and the cast-iron splice, instead of the mass of lead which had been specified for, and which he had used with success in another case of an estuary crossing, in which the masonry piers were supported by bearing-piles wholly of cast iron, and driven precisely as timber piles. This was the bridge over the estuary of the Swale on the Sittingbourne and Sheerness railway, which he constructed in the years 1857–58. The length of this bridge was a little over 600 feet, and the width of the platform was 30 feet 3 inches. It carried a single line of railway and a turnpike road. There were twelve openings; eleven of 47 feet span, and an opening span of 60 feet clear in the centre of the channel. The piers were of brick in cement, resting on piled foundations; the depth of the channel at high water was 37 feet, and at low water, 18 feet. The bearing-piles were of cast iron, the cross section of which measured 16 inches by 10 inches, and consisted of a longitudinal web, extending 16 inches, crossed by two transverse webs of 10 inches each; the cross section, which was adopted from some similar works in the north of England, was a very advantageous one, as the disposition of the longitudinal and transverse webs caused it to drive particularly true, and afforded a large periphery of frictional bearing surface. These cast-iron piles were 42 feet in length for the piers of the central opening, and 32 feet for the other piers; the 42-feet piles were driven 25 feet into the bed of the channel, which was of London clay; and thence to their heads, which were driven down level with low-water mark, was 17 feet; the heads of the piles were rectangles of 16 inches by 10 inches, formed by flanges filling up the spaces between the longitudinal and transverse webs. These piles were designed to be driven 4 feet apart from centre to centre, but were really driven somewhat closer. At low-water mark they were surmounted by two courses of Yorkshire flagstones, above which the piers were carried up in brick in cement. In driving the piles a mass of lead was interpolated between the monkey and the pile-head,

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and not a pile was broken in driving. He had referred to this bridge as an example of a description of piled foundations which, though not very infrequent before cylinders and screw-piles came into such general use, had never before been noticed in the Proceedings.

Several questions were asked regarding the stoning of the channel under the Barmouth viaduct. The stoning was continuous, and had settled down to the bed of gravel. Masses of rough stone when thus thrown down, in a rapid current with a bottom of shifting sand, very shortly found their way to the bottom of the sand if shallow, as in this case; or if the sand were deep, to a depth at which they ceased to be liable to disturbance; and if the stoning were carried to a proper extent, it formed a settled mass incorporated with sand, and incapable of further movement or settlement.

As regarded the doubts expressed as to the utility of the supplementary bearing discs, and the apprehension lest, in case of a settlement of the stoning, their unsupported weight might dangerously affect the stability of the structure, he might observe that the supporting power of the area of the screws was more than ample for the support of the bridge, and therefore that the stability of the structure was wholly independent of these discs, preserving their bearing on the stoning. He applied them, however, because it was the usual practice to bolt balks of timber, resting on the stoning, on each side of the single row of piles which usually formed the piers of estuary viaducts similarly circumstanced; and as the tripoidal arrangement of the pier-piles of the Barmouth viaduct precluded the application of the expedient usual in such cases, he adopted the discs as an equivalent. As regarded the second part, even supposing that the stoning did settle to the extent of depriving these discs of its support, their weight was too insignificant to affect the coefficient of safety allowed in calculating the area of the screws.

Some apprehensions were expressed lest the tripoidal arrangement of the piles should cause more weight to be thrown on one pile than another. He could not understand the force of the objection, for it was manifest that the effect of such an arrangement must be to preclude the possibility of unequal bearings. Each of the three legs of each tripod must necessarily bear one-third of the load at the apex of such tripod, which was formed by a single casting forming one of the two supports of the transverse girders that supported the longitudinal girders of the bridge. Under such an arrangement an unequal bearing on the piles was an impossibility. The primary object, however, of the arrangement of the piles in the pier was to distribute the weight on the piers over a larger

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area of the thin crust of gravel than could alone be depended on for the support of the bridge.

Mr. Hemans had observed that there was at least a third variety of the telescope draw-bridge, in addition to the under and over arrangements adopted respectively for the opening spans of the Dovey and the Barmouth viaducts, namely, the arrangement in which the sliding portion was of rather more than double the length of the opening, and was protruded horizontally, the gap left in the line by its protrusion being filled up by a section of the way, wheeled in laterally from a recess in which it remained when the bridge was retracted; and there had been also mentioned an old and very cumbrous arrangement in which the opening span was withdrawn and wheeled off, without losing its parallelism, on rails laid at an angle of 45° with the axis of the bridge. He was acquainted with both these arrangements, but considered them obsolete, being, as they were so manifestly, inferior to more modern arrangements, in respect of first cost, space occupied and moved through, and facility and rapidity of manipulation.

There was a simple and inexpensive arrangement for small spans, adopted for canal crossings on some of the early Dutch railways.[1] Here each rail was supported on a hinged bracket, the extremity of the brackets being connected by a link, so that the whole formed an articulated parallelogram, capable of being opened horizontally to a rectangle, or completely closed like a parallel ruler. When a boat passed the parallel ruler, so to speak, was closed. When a train was to pass the ruler was extended fully, the extremities of the principal brackets resting on a sill on the farther side of the opening. This arrangement was obviously only applicable for small spans, and would scarcely pass the Board of Trade inspectors in this country. The same might be said of the various contrivances for military draw-bridges.

It appeared to him that for railway purposes only two species of draw-bridge were suitable, the swing-bridge and the telescope-bridge; and that this latter class being the cheapest to construct, and the more facile and rapid in manipulation, should be adopted in all cases where two contiguous openings were not required, except when the span of the opening was exceptionally great; also that of the two varieties of the telescope-draw-bridge, the over and the under, the former was much the best, for the reasons stated in the body of his Paper.

In reply to Mr. Brunlees, he might observe that although

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. iii., p. 194.

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Mr. Piercy (whom he had succeeded as Engineer to the Welsh Coast railway) had designed a viaduct for carrying that line across the Mawddach, at Barmouth, the viaduct actually constructed three years later was designed and calculated 'de novo,' and differed from that projected by Mr. Piercy in the following essential particulars: 1st. The sites of the two were not identical. The different positions of the axis of the two designs on plan was trifling, but it sufficed to affect the engineering design of the bridge. For example, the two northernmost piers in Mr. Piercy's design were of screw piles, while in his bridge these piers rested on naked rock, and were consequently composed of cylinders. 2nd. As regarded the treatment of the estuary, Mr. Conybeare's viaduct afforded a water-way of over 2,600 feet, and Mr. Piercy's design not more than half or two-thirds as much. 3rd. As regarded the adaptation of the design to the specialities of the site. In Mr. Piercy's design the piers, the spans, the superstructure, and the character and scantling of the piles were identical, except in the opening span, throughout the entire length of the bridge; whereas in the bridge carried out by him the material, the spans, and the diameter of the cast-iron piles were varied according to the depth of the channel. 4th. The superstructure was altogether different. The three girders which formed the superstructure of each span of the actual viaduct were all three dissimilar from each other, and no one of the three was the same as any girder in Mr. Piercy's design, and the strength of the superstructure as executed was about 50 per cent. in excess of that designed by Mr. Piercy. 5th. As regarded the piers. Each of the piers in Mr. Piercy's design consisted of a number of piles set in a row in the usual stereotyped fashion; while in his there were six piles in each pier arranged in two equilateral triangular groups. It would be impossible to conceive two arrangements more utterly distinct from one another. The position of the screw discs on the piles, 8 feet above the lower extremity of the latter, was not claimed as original, and he was informed that this arrangement was also adopted—at Mr. Brunlees' suggestion—in Mr. Piercy's design. As stated in the Paper, a drawbridge, similar in principle to that adopted at Barmouth, had been previously put up at Rhyl, to carry the turnpike road over the river; and Mr. Brereton had stated that a bridge on the same principle was erected across the river Towey in 1852. The system had answered most satisfactorily at Barmouth, for during the four or five years the bridge had been in use, it had been worked with the greatest ease and rapidity by a single man.

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Mr. W. BELL observed, through the Secretary, that when the Clevedon pier was being constructed, he calculated its strength and stiffness, considering it as a continuous beam of eight spans. The investigation of such a beam was unusual, and the diagrams (Plate 12A) showed the stresses and deflections at the different points] with each span loaded, or with the whole beam loaded. He had also attempted to estimate the effect of the arches, assuming them to be brackets, but as they wanted the top member to constitute them brackets, their advantage had been rather over-estimated.

This investigation might be divided into three parts:

1. The strength of the girders, assuming them to be continuous beams of uniform section, of eight equal spans, and loaded on any or all of the spans.

2. The stiffness of the piers, considering them as flexible columns with wrought-iron arches or brackets projecting from the upper parts.

3. The combined action of the piers and continuous beams, assuming that these latter rested on the ends of the brackets, and on the uprights of the piers.

The first part of the investigation being general, the formulæ were readily applicable to the case of a continuous beam of any number of equal spans. From the construction of the equations it followed that the stresses and deflections, for two or more spans loaded, were the algebraical sums of the stresses and deflections for each separate loaded span.

In regard to the second and third parts of the investigation, it was necessary to premise, that although the consequences of assuming the continuous beam to be supported on piers and flexible brackets had been worked out in detail, the results were only approximate as regarded the Clevedon pier; since, in that structure, the top members of the brackets must be considered as included in the lower web of the continuous beam itself. The effect of the brackets was therefore probably somewhat over-estimated, though in order to make some allowance for the want of the top member, the sectional areas of the top and bottom members of the brackets had been taken as each equal to only one half the sectional area of the wrought-iron arches.

The diagram of the combined action of the piers and continuous beams, Fig. 2, Plate 12A, showed considerable diminution of the stresses. It might be considered as exhibiting a step in the passage of a continuous beam of eight spans into a continuous beam of twenty-four spans. It was evident that if the brackets were perfectly rigid, the continuous beam supported on their ends

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and on the uprights of the piers, would be one of twenty-four spans.

1. STRESSES AND DEFLECTIONS OF A CONTINUOUS BEAM OF ANY NUMBER OF EQUAL SPANS, AND OF UNIFORM SECTION.

Let l be the length of each of the equal spans, I the moment of inertia of the cross section of the beam, E the denominator of the fraction, expressing the extension or compression of the unit of length of the material of the beam by a force of 1 ton per square inch. Also, let inches and tons be the units of length and weight, and, reckoning from the unloaded end of the beam, let r_0, r_1, r_2, &c., be the pressures on the piers: x and y the running co-ordinates, horizontal and vertical, of the elastic curves, the origin being taken at the commencement of each span, $+ x$ measured towards the loaded end, and $+ y$ downwards.

Supposing the beam unloaded on all spans up to the $(m + 1)$th, then for some span nearer than this to the unloaded end, say the fourth, the equation of stress, expressing that the moment of elasticity was equal to the moment of the outward forces, was

$$\mathrm{E\,I} \cdot \frac{d^2y}{dx^2} = -(r_0 + r_1 + r_2 + r_3)\,x - (3r_0 + 2r_1 + r_2)\,l.$$

By integrating this equation the value of $\frac{dy}{dx}$ was obtained, a constant, say c_4, being added, which expressed the value of $\frac{dy}{dx}$ when $x = 0$.

A second integration gave y, the ordinate to the curve of deflection, or elastic curve. The tops of the piers being supposed all at the same level, no new constant was introduced, since $y = 0$ when $x = 0$.

In this manner equations were obtained for each span, the undetermined quantities being the constants c_1, c_2, &c., and the pressures on the piers r_0, r_1, r_2, &c.

To determine these, one equation of condition for each span was obtained by making $y = 0$ when $x = l$, and another by making the value of $\frac{dy}{dx}$ when $x = l$ in one span, equal to the value of $\frac{dy}{dx}$ when $x = 0$ in the span immediately following.

The elastic curve of the beam, considered as a single curve, was discontinuous, the separate parts for each span being fitted

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together by making the ordinates and tangents coincide at the ends of the spans. This discontinuity was the consequence of the abrupt change of the bending moment at the piers. Each span had a separate equation for its elastic curve, and the only part of this curve which was applicable to the physical state of the beam was the part lying within the limits of the span: the branches of the curve lying outside these limits, not being adapted to the change of bending moment, were inapplicable to the actual state of the beam. A different equation thus became necessary, and the equation of the adjoining span, found as above, represented the state of the beam within the limits of that span, and so on for the other spans.

By writing down the equations of condition in a series beginning with the end span, and eliminating, it was found that r_1, r_2, &c., c_1, c_2, &c., could be expressed in terms of r_0.

Thus

$$\begin{aligned}
r_1 &= -6\,r_0 & c_1 &= +\frac{l^2}{6}r_0 \\
r_2 &= +24\,r_0 & c_2 &= -2\frac{l^2}{6}r_0 \\
r_3 &= -90\,r_0 & c_3 &= +7\frac{l^2}{6}r_0 \\
r_4 &= +336\,r_0 & c_4 &= -26\frac{l^2}{6}r_0 \\
r_5 &= -1254\,r_0 & c_5 &= +97\frac{l^2}{6}r_0 \qquad \ldots\ldots (A). \\
r_6 &= +4680\,r_0 & c_6 &= -362\frac{l^2}{6}r_0 \\
r_7 &= -17466\,r_0 & c_7 &= +1351\frac{l^2}{6}r_0 \\
r_8 &= +65184\,r_0 & c_8 &= -5042\frac{l^2}{6}r_0 \\
\&c. &= \&c. & \&c. &= \&c.
\end{aligned}$$

This series might be continued to any extent, the law of successive derivation being,

$$\begin{aligned}
r_k + 4\,r_{k+1} + r_{k+2} &= 0 \\
c_k + 4\,c_{k+1} + c_{k+2} &= 0
\end{aligned} \qquad \ldots\ldots (B).^1$$

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[1] To show this, write down the equations of the $(k+1)$th span,

$$EI\frac{d^2y}{dx^2} = -(r_k + r_{k-1} + r_{k-2} + \&c.)\,x - (r_{k-1} + 2\,r_{k-2} + 3\,r_{k-3} + \&c.)\,l$$

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Calling t the tension or compression of the material of the beam per square inch at the distance k_0 from the neutral axis, and R the radius of curvature of the elastic curve, then

$$\frac{E}{R} = \frac{t}{k_0};$$

$$E I \frac{dy}{dx} = -(r_k + r_{k-1} + r_{k-2} + \&c.)\frac{x^2}{2} - (r_{k-1} + 2r_{k-2} + 3r_{k-3} + \&c.) l x + c_{k+1}$$

$$E I y = -(r_k + r_{k-1} + r_{k-2} + \&c.)\frac{x^3}{6} - (r_{k-1} + 2r_{k-2} + 3r_{k-3} + \&c.)\frac{l x^2}{2} + c_{k+1} x.$$

Making $y = 0$ in this last when $x = l$,

$$c_{k+1} = \frac{l^2}{6}(r_k + 4r_{k-1} + 7r_{k-2} + 10r_{k-3} + \&c.) \quad . \quad . \quad . \quad (1).$$

In the equations for the $(k+2)$nd span, the constant introduced, or c_{k+2}, was the value of $\frac{dy}{dx}$ in the above equation for $\frac{dy}{dx}$, when $x = l$, or

$$c_{k+2} = c_{k+1} - \frac{l^2}{2}(r_k + 3r_{k-1} + 5r_{k-2} + 7r_{k-3} + \&c.) \quad . \quad . \quad (2).$$

Substituting in this, the above value of c_{k+1}, there resulted

$$c_{k+2} = -\frac{l^2}{6}(2r_k + 5r_{k-1} + 8r_{k-2} + 11r_{k-3} + \&c.). \quad . \quad . \quad (3).$$

Writing $k+1$ for k in (1),

$$c_{k+2} = \frac{l^2}{6}(r_{k+1} + 4r_k + 7r_{k-1} + 10r_{k-2} + \&c.).$$

Equating these two values of c_{k+2},

$$r_{k+1} = -6(r_k + 2r_{k-1} + 3r_{k-2} + 4r_{k-3} + \&c.).$$

Writing in this last equation $k-1$ and $k+1$ for k,

$$-\frac{r_k}{6} = r_{k-1} + 2r_{k-2} + 3r_{k-3} + \&c.$$

$$\frac{-r_{k+1}}{6} = r_k + 2r_{k-1} + 3r_{k-2} + 4r_{k-3} + \&c.$$

$$\frac{-r_{k+2}}{6} = r_{k+1} + 2r_k + 3r_{k-1} + 4r_{k-2} + 5r_{k-3} + \&c.$$

deducting

$$\frac{-r_{k+2} + r_{k+1}}{6} = r_{k+1} + r_k + r_{k-1} + r_{k-2} + \&c.$$

$$\frac{-r_{k+1} + r_k}{6} = r_k + r_{k-1} + r_{k-2} + \&c.$$

deducting again

$$\frac{-r_{k+2} + r_{k+1}}{6} - \frac{-r_{k+1} + r_k}{6} = r_{k+1},$$

or

$$r_k + 4r_{k+1} + r_{k+2} = 0.$$

Again, from (1) writing $k-1$ for k,

$$c_{k+1} = \frac{l^2}{6}(r_k + 4r_{k-1} + 7r_{k-2} + \&c.)$$

$$c_k = \frac{l^2}{6}(r_{k-1} + 4r_{k-2} + 7r_{k-3} + \&c.)$$

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and since $\frac{1}{R}$ might be taken as equal to $\frac{d^2y}{dx^2}$,

$$E\frac{d^2y}{dx^2} = \frac{t}{k_0}.$$

Writing down the equations of stress in a series beginning with the end span, and substituting the above value of $E\frac{d^2y}{dx^2}$, and the values of r_1, r_2, r_3, &c., from equations (A), the equations of stress became

$$\begin{aligned}
&\text{1st span,} \quad t = \frac{k_0}{I} r_0 \cdot x \\
&\text{2nd ,,} \quad t = \frac{k_0}{I} r_0 (+5x - l) \\
&\text{3rd ,,} \quad t = \frac{k_0}{I} r_0 (-19x + 4l) \\
&\text{4th ,,} \quad t = \frac{k_0}{I} r_0 (+71x - 15l) \\
&\text{5th ,,} \quad t = \frac{k_0}{I} r_0 (-265x + 56l)
\end{aligned} \qquad \ldots . (C).$$

---

deducting

$$c_{k+1} - c_k = \frac{l^2}{6}(r_k + 3r_{k-1} + 3r_{k-2} + 3r_{k-3} + \&c.) \quad . \quad . \quad . \quad (4).$$

From (3) in the same manner,

$$c_{k+2} = -\frac{l^2}{6}(2r_k + 5r_{k-1} + 8r_{k-2} + 11r_{k-3} + \&c.)$$

$$c_{k+1} = -\frac{l^2}{6}(2r_{k-1} + 5r_{k-2} + 8r_{k-3} + 11r_{k-4} + \&c.)$$

deducting

$$c_{k+2} - c_{k+1} = -\frac{l^2}{6}(2r_k + 3r_{k-1} + 3r_{k-2} + 3r_{k-3} + \&c.) \quad . \quad (5).$$

Adding (4) and (5),

$$c_{k+2} - c_k = -\frac{l^2}{6} r_k \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

Adding the above value of c_{k+2} to the first value of c_{k+1}, and multiplying by 3,

$$3c_{k+2} + 3c_{k+1} = -\frac{l^2}{6}(3r_k + 3r_{k-1} + 3r_{k-2} + \&c.);$$

and adding this equation to (4),

$$3c_{k+2} + 4c_{k+1} - c_k = -\frac{l^2}{6} \cdot 2r_k \quad . \quad . \quad . \quad . \quad (7).$$

From (6) and (7),

$$2c_{k+2} - 2c_k = 3c_{k+2} + 4c_{k+1} - c^k,$$

or

$$c_{k+2} + 4c_{k+1} + c_k = 0.$$

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6th span, $t = \frac{k_0}{I} r_0 (+ 989\,x - 209\,l)$

7th ,, $t = \frac{k_0}{I} r_0 (- 3691\,x + 780\,l)$

8th ,, $t = \frac{k_0}{I} r_0 (+ 13775\,x - 2911\,l)$

&c. &c.

By substituting $k_0 E \frac{d^2 y}{dx^2}$ for t, and integrating the above equations twice, introducing the above values of the constants (c), the equations of the elastic curves became

1st span, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{1 - \left(\frac{x}{l}\right)^2\right\}$

2nd ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{+ 5\left(\frac{x}{l}\right)^2 - 3\frac{x}{l} - 2\right\}$

3rd ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{- 19\left(\frac{x}{l}\right)^2 + 12\frac{x}{l} + 7\right\}$

4th ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{+ 71\left(\frac{x}{l}\right)^2 - 45\frac{x}{l} - 26\right\}$

5th ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{- 265\left(\frac{x}{l}\right)^2 + 168\frac{x}{l} + 97\right\}$ . . . (D).

6th ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{+ 989\left(\frac{x}{l}\right)^2 - 627\frac{x}{l} - 362\right\}$

7th ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{- 3691\left(\frac{x}{l}\right)^2 + 2340\frac{x}{l} + 1351\right\}$

8th ,, $y = \frac{l^3}{6EI} r_0 \frac{x}{l} \left\{+ 13775\left(\frac{x}{l}\right)^2 - 8723\frac{x}{l} - 5042\right\}$

&c. &c.

If the beam were now supposed to be loaded on the $(m+1)$th span, with a weight Q uniformly distributed over the length of the span, the values of r and c, and the equations of stress and deflection for the first m spans, would be the same as those above; but for the $(m+1)$th and subsequent spans, the above equations would require modification. Calling r_m, r_{m+1}, &c., c_{m+2}, c_{m+3}, &c., the values of r and c for the $(m+1)$th and subsequent spans, as given by the above equations, let these quantities after the loading be $r_m + \rho_0$, $r_{m+1} + \rho_1$, &c., $c_{m+2} + \gamma_2$, $c_{m+3} + \gamma_3$, &c.; then, applying the same

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process as that above described to find r_1, r_2, &c., c_2, &c., there resulted

$$\rho_0 = +\frac{1}{4}Q$$

$$\rho_1 = +\frac{7}{4}Q \qquad \gamma_1 = 0$$

$$\rho_2 = -\frac{18}{4}Q \qquad \gamma_2 = +\frac{1}{24}Ql^2$$

$$\rho_3 = +\frac{66}{4}Q \qquad \gamma_3 = -\frac{5}{24}Ql^2$$

$$\rho_4 = -\frac{246}{4}Q \qquad \gamma_4 = +\frac{19}{24}Ql^2$$

$$\rho_5 = +\frac{918}{4}Q \qquad \gamma_5 = -\frac{71}{24}Ql^2$$

&c. &c.

Except ρ_0 and ρ_1 which were under the direct influence of the weight, the law of successive derivation (B) applied to the quantities ρ and γ.

These additions ρ_0, ρ_1, &c., to the values of r_m, r_{m+1}, &c., would evidently introduce corresponding additional terms into the equations of stress (C), and by writing down these equations, taking into account the action of the new force Q introduced, it would become apparent that there were also terms added by the direct action of Q. For instance, forming the equation of the $(m+1)$th span, the term added by the alteration of r_m into $r_m + \rho_0$ was $-\rho_0 x$, or $-\frac{Q}{4}x$, and the term added by the direct action of Q was $+\frac{Q}{l}x \cdot \frac{x}{2}$, or $\frac{Q}{l} \cdot \frac{x^2}{2}$. For the $(m+2)$nd span, the terms added by the alteration of r_m and r_{m+1} were $-\rho_0(l+x) - \rho_1 x$, or $-\frac{Q}{4}(l+x) - \frac{7}{4}Qx$, and the term added by the direct action of Q was $+Q\left(\frac{l}{2}+x\right)$. Thus it was found that the added terms were:

$$\text{For the } (m+1)\text{th span, } +\frac{k_0}{I}\frac{Q}{4}\left(+2\frac{x^2}{l}-x\right)$$

$$,, \quad (m+2)\text{nd} \quad ,, \quad +\frac{k_0}{I}\frac{Q}{4}(-4x+l) \qquad \ldots (C').$$

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c. &c.

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Loaded on the	r_0	r_1	r_2	r_3	r_4	r_5	r_6	r_7	r_8
1st span only	+18817	+28330	− 4680	+ 1254	− 336	+ 90	− 24	+ 6	− 1
2nd „	− 2131	+23650	+24904	− 3762	+ 1008	− 270	+ 72	− 18	+ 3
3rd „	+ 571	− 3426	+24568	+24658	− 3696	+ 990	− 264	+ 66	− 11
4th „	− 153	+ 918	− 3672	+24634	+24640	− 3690	+ 984	− 246	+ 41
5th „	+ 41	− 246	+ 984	− 3690	+24640	+24634	− 3672	+ 918	− 153
6th „	− 11	+ 66	− 264	+ 990	− 3696	+24658	+24568	− 3426	+ 571
7th „	+ 3	− 18	+ 72	− 270	+ 1008	− 3762	+24904	+23650	− 2131
8th „	− 1	+ 6	− 24	+ 90	− 336	+ 1254	− 4680	+28330	+18817
All spans loaded	+17136	+49280	+41888	+43904	+43232	+43904	+41888	+49280	+17136

The equations of stress, and of the elastic curves, were formed from these values, and from equations (C), (C′), (D), (D′), in the manner already explained.

Although the equations of stress and deflection with all the spans loaded could be made by the addition of those for each span loaded, it was easier to construct these equations directly from the above values of r_0, r_1, &c., which, it would be observed, were formed by algebraical addition.

The equations of stress were:

$$\text{1st, or end span, } t = \frac{k_0}{I} Q l \cdot \frac{x}{l} \cdot \left\{ + \cdot 5 \frac{x}{l} - \cdot 394 \right\}$$

$$\text{2nd span} \quad t = \frac{k_0}{I} Q l \left\{ + \cdot 5 \left(\frac{x}{l}\right)^2 - \cdot 5283 \frac{x}{l} + \cdot 1057 \right\}$$

$$\text{3rd ,,} \quad t = \frac{k_0}{I} Q l \left\{ + \cdot 5 \left(\frac{x}{l}\right)^2 - \cdot 4922 \frac{x}{l} + \cdot 0774 \right\} \quad \text{. . (E).}$$

$$\text{4th ,,} \quad t = \frac{k_0}{I} Q l \left\{ + \cdot 5 \left(\frac{x}{l}\right)^2 - \cdot 5025 \frac{x}{l} + \cdot 0852 \right\} .$$

And for the elastic curves:

$$\text{1st span } y = \frac{Q l^3}{24 E I} \cdot \frac{x}{l} \cdot \left\{ \left(\frac{x}{l}\right)^3 - 1 \cdot 577 \left(\frac{x}{l}\right)^2 + \cdot 577 \right\}$$

$$\text{2nd ,, } y = \frac{Q l^3}{24 E I} \cdot \frac{x}{l} \cdot \left\{ \left(\frac{x}{l}\right)^3 - 2 \cdot 1132 \left(\frac{x}{l}\right)^2 + 1 \cdot 2684 \frac{x}{l} - \cdot 1544 \right\} \quad \text{(F).}$$

$$\text{3rd ,, } y = \frac{Q l^3}{24 E I} \cdot \frac{x}{l} \cdot \left\{ \left(\frac{x}{l}\right)^3 - 1 \cdot 9688 \left(\frac{x}{l}\right)^2 + \cdot 9288 \frac{x}{l} + \cdot 0406 \right\}$$

$$\text{4th ,, } y = \frac{Q l^3}{24 E I} \cdot \frac{x}{l} \cdot \left\{ \left(\frac{x}{l}\right)^3 - 2 \cdot 0100 \left(\frac{x}{l}\right)^2 + 1 \cdot 0224 \frac{x}{l} - \cdot 006 \right\}$$

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Applying these formulæ to the Clevedon pier, the values of l, Q, &c., for one girder, were:

$$l = 1{,}200$$
$$k_0 = 21$$
$$\mathrm{I} = 13{,}454$$
$$\mathrm{Q} = 50$$
$$\mathrm{E} = 8{,}000.$$

The value of E for solid wrought iron was about 10,000, but 8,000 had been used, to allow for loss of strength by rivetting.

Subdividing each span into ten equal parts, and making

$$\frac{x}{l} = \cdot 1, \cdot 2, \cdot 3, \text{ \&c.},$$

the stresses and deflections at each of these points were obtained from the above formulæ by substituting in them the values of l, k_0, &c.

Fig. 1, Plate 12A, showed the stresses and deflections with any one of the spans loaded uniformly, and also with all the spans loaded. For the sake of comparison, similar results for a continuous beam of two spans only, and for a beam of one span, with the same values of l, k_0, I, Q, and E, were also shown.

Putting $\phi\left(\frac{x}{l}\right)$ for that part of the equations (C), (C'), and (E), which depended only on $\frac{x}{l}$, and $\psi\left(\frac{x}{l}\right)$ for the like part in equations (D), (D'), and (F), the equations of stress and deflection might be thus written:

$$t = \frac{k_0}{\mathrm{I}} \mathrm{Q}\, l \cdot \phi\left(\frac{x}{l}\right)$$

$$y = \frac{\mathrm{Q}\, l^3}{24\, \mathrm{E\, I}} \cdot \psi\left(\frac{x}{l}\right)$$

Denoting by t', y', l', k_0', I', Q', and E', corresponding quantities for another continuous beam of different length, section, loading, &c., but of the same number of equal spans, then

$$t' = \frac{k_0'}{\mathrm{I}'} \mathrm{Q}'\, l' \cdot \phi\left(\frac{x}{l}\right)$$

$$y' = \frac{\mathrm{Q}'\, l'^3}{24\, \mathrm{E}'\, \mathrm{I}'} \psi\left(\frac{x}{l}\right);$$

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and by eliminating $\phi\left(\frac{x}{l}\right)$ and $\psi\left(\frac{x}{l}\right)$,

$$t' = t \cdot \frac{k_0'}{k_0} \cdot \frac{Q'}{Q} \cdot \frac{l'}{l} \cdot \frac{I}{I'}$$

$$y' = y \cdot \frac{Q'}{Q}\left(\frac{l'}{l}\right)^3 \cdot \frac{E}{E'} \cdot \frac{I}{I'}$$

Hence, by applying these ratios to the values of t and y on the diagram, the corresponding values t' and y' for any continuous beam of eight equal spans might be readily found.

Suppose now the beam to have the first n spans unloaded, and the $(n+1)$ th span to be loaded with a weight P, which acted on a point of the beam whose distance from the origin of co-ordinates for that span was (a).

Then the values of r_1, r_2, &c., up to r_{n-1}, c_1, c_2, &c., up to c_n, would still be given by equations (A), and the equations of stress and deflection for the n spans by (C) and (D).

At the $(n+1)$ th span, for that part of the elastic curve between P and the origin, the value of c_{n+1} would also be given by (A), since it was equal to the value of $\frac{dy}{dx}$ for the nth span when $x = l$.

There being an abrupt change of the bending moment at the point where P acted, there was discontinuity in the curve at that point, and the parts of the elastic curve of the beam lying to the right and left of P could not be expressed by the same algebraical equation. When x was greater than a, the term $+ P(x - a)$ was added to the equation of stress; and in integrating this equation to find the value of y, two new constants were introduced: one of these was an addition to c_{n+1}, and the other was the value of y when $x = 0$, since the condition $y = 0$ when $x = 0$ no longer held good for this curve. The two constants were determined by fitting the two pieces of curve together, so that the values of y and $\frac{dy}{dx}$ at the point where the weight P acted, might be the same for that piece of the curve where x was less than a, as for the piece where x was greater than a. The values of r_n, r_{n+1}, &c., c_{n+1}, as already explained, c_{n+2}, &c., and the equations of stress and of the elastic curve for the $(n+1)$ th and subsequent spans, would all be altered. Calling the additions to r_n, r_{n+1}, &c., $\sigma_0, \sigma_1, \sigma_2$, &c., and the addition to c_{n+1}, when x was greater than a, δ_1, the additions to c_{n+2}, c_{n+3}, &c., δ_2, δ_3, &c., it was found by the process explained above, that,

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$$\sigma_0 = \mathrm{P}\left(1-\frac{a}{l}\right)^3 \qquad = \mathrm{P}\cdot\left(1-\frac{a}{l}\right)^3$$

$$\sigma_1 = \mathrm{P}\cdot\left\{\begin{matrix}1-7\sigma_0\\ +3\left(1-\frac{a}{l}\right)\left(2-\frac{a}{l}\right)\end{matrix}\right\} = 6\,\mathrm{P}\cdot\frac{a}{l}\left\{+2-3\frac{a}{l}+\frac{7}{6}\left(\frac{a}{l}\right)^2\right\}$$

$$\sigma_2 = \mathrm{P}\cdot\left\{\begin{matrix}1-19\sigma_0-7\sigma_1\\ +3\left(2-\frac{a}{l}\right)\left(3-\frac{a}{l}\right)\end{matrix}\right\} = 6\,\mathrm{P}\cdot\frac{a}{l}\left\{-7+12\frac{a}{l}-5\left(\frac{a}{l}\right)^2\right\}$$

$$\sigma_3 = \mathrm{P}\cdot\left\{\begin{matrix}1-37\sigma_0-19\sigma_1-7\sigma_2\\ +3\left(3-\frac{a}{l}\right)\left(4-\frac{a}{l}\right)\end{matrix}\right\} = 6\,\mathrm{P}\cdot\frac{a}{l}\left\{+26-45\frac{a}{l}+19\left(\frac{a}{l}\right)^2\right\}$$

$$\sigma_4 = \mathrm{P}\cdot\left\{\begin{matrix}1-61\sigma_0-37\sigma_1-19\sigma_2-7\sigma_3\\ +3\left(4-\frac{a}{l}\right)\left(5-\frac{a}{l}\right)\end{matrix}\right\} = 6\,\mathrm{P}\cdot\frac{a}{l}\left\{-97+168\frac{a}{l}-71\left(\frac{a}{l}\right)^2\right\}$$

&c. &c.

$$\delta_1 = +\mathrm{P}\frac{a^2}{2}$$

$$\delta_2 = +\frac{\mathrm{P}}{6}\left(+3\,al-6\,a^2+3\frac{a^3}{l}\right)$$

$$\delta_3 = +\frac{\mathrm{P}}{6}\left(-12\,al+21\,a^2-9\frac{a^3}{l}\right)$$

$$\delta_4 = +\frac{\mathrm{P}}{6}\left(+45\,al-78\,a^2+33\frac{a^3}{l}\right)$$

&c. &c.

The law (B) applying to these quantities, except for the span on which P acted, and the adjoining one.

By these additions to r_n, r_{n+1}, &c., and by the direct action of P, the following terms were added to the equations of stress (C):

$(n+1)$th span $x < a, \quad -\sigma_0 x$

,, ,, $x > a, \quad -\sigma_0 x + \mathrm{P}(x-a)$

$(n+2)$nd ,, $\left\{\begin{matrix}-(\sigma_0+\sigma_1)x-\sigma_0 l\\ +\mathrm{P}x+\mathrm{P}(l-a)\end{matrix}\right\}$ .... (C'').

$(n+3)$rd ,, $\left\{\begin{matrix}-(\sigma_0+\sigma_1+\sigma_2)x-(\sigma_1+2\sigma_0)l\\ +\mathrm{P}x+\mathrm{P}(2l-a)\end{matrix}\right\}$

$(n+4)$th ,, $\left\{\begin{matrix}-(\sigma_0+\sigma_1+\sigma_2+\sigma_3)x-(\sigma_2+2\sigma_1+3\sigma_3)l\\ +\mathrm{P}x+\mathrm{P}(3l-a)\end{matrix}\right\}$

&c. &c.

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Integrating these terms twice, having added after the first integrations the above values of δ_1, δ_2, &c., to the second sides of the equations to which they corresponded, it was found that the following terms were added to the equations of the elastic curve (D):

$$(n+1)\text{th span } x<a, -\frac{P a^3}{6}\cdot\left(1-\frac{a}{l}\right)^3\left(\frac{x}{l}\right)^3$$

$$,, \qquad x>a, +\frac{P a^3}{6}\left\{\left(\frac{x-a}{l}\right)^3-\left(\frac{l-a}{l}\right)^3\left(\frac{x}{l}\right)^3\right\}$$

$$(n+2)\text{nd span}, +\frac{P a^3}{6}\cdot\frac{x}{l}\left\{\begin{array}{l}(1-\sigma_0-\sigma_1)\left(\frac{x}{l}\right)^2\\ +3\left(1-\sigma_0-\frac{a}{l}\right)\frac{x}{l}\\ +3\left(1-\frac{a}{l}\right)^2-3\sigma_0\end{array}\right\} \quad \ldots\ldots (D'').$$

$$(n+3)\text{rd span}, +\frac{P a^3}{6}\cdot\frac{x}{l}\cdot\left\{\begin{array}{l}(1-\sigma_0-\sigma_1-\sigma_2)\left(\frac{x}{l}\right)^2\\ +3\left(2-2\sigma_0-\sigma_1-\frac{a}{l}\right)\frac{x}{l}\\ +3\left\{1-4\sigma_0-\sigma_1+\left(1-\frac{a}{l}\right)\left(3-\frac{a}{l}\right)\right\}\end{array}\right\}$$

$$(n+4)\text{th span}, +P\frac{a^3}{6}\cdot\frac{x}{l}\cdot\left\{\begin{array}{l}(1-\sigma-\sigma-\sigma_2-\sigma_3)\left(\frac{x}{l}\right)^2\\ +3\left(3-3\sigma_0-2\sigma_1-\sigma_2-\frac{a}{l}\right)\cdot\frac{x}{l}\\ +3\left\{4-9\sigma-4\sigma_1-\sigma_2+\left(1-\frac{a}{l}\right)\left(5-\frac{a}{l}\right)\right\}\end{array}\right\}$$

&c. &c.

These expressions might be reduced by substituting the values of σ_0, σ_1, &c., but they were as easy of computation in their present form.

It was worthy of remark that with the exception of the first equations in (C), (C',) (D), (D'), the coefficients of the terms followed the law (B). For example, taking the 4th, 5th, and 6th equations (D):

$$\begin{aligned}+71+4(-265)+989 &= 0\\ -45+4\times 168-627 &= 0\\ -26+4\times 97-362 &= 0\end{aligned}$$

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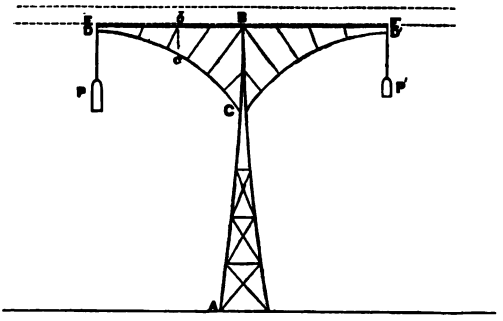
Probably this could be shown to be a consequence of (B). It meant that for the same values of $\frac{x}{l}$ in the different spans t and y themselves followed this law, as would be seen on examination of the diagrams Fig. 1, Plate 12A.

2. Stiffness of the piers and arches.

If, in the annexed Fig. 1, the bracket B C D E were considered as fixed along B C, so as to be immovable there, the deflection at E from the action of a weight P could be ascertained as follows:

Fig. 1.



Suppose the top and bottom webs B b E and C c D to be of equal sectional areas, and the space between these so filled in with trussing that the whole acted as an ordinary girder. To form the equation of stress, let the area of each of these webs be A, and let B E = a, B C = d, and E D = e. Also let B b, the horizontal distance from B of any point b, be called x, and the deflection, or ordinate of the elastic curve there, y.

If $b\,c$ were the distance between the centres of the top and bottom webs, the value of I, the moment of inertia of the cross section, might be taken as equal to

$$2\,\mathrm{A}\left(\frac{b\,c}{2}\right)^2, \text{ or } \frac{\mathrm{A}\cdot(b\,c)^2}{2},$$

and the equation of stress was

$$\mathrm{E}\cdot\mathrm{A}\cdot(b\,c)^2\frac{d^2y}{dx^2} = 2\,\mathrm{P}(a-x).$$

Assuming the curve D c C to be a portion of a parabola whose vertex is D,

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$$b\,c - e : d - e : : \mathrm{E}\,b^2 : \mathrm{E}\,\mathrm{B}^2 : : (a - x)^2 : a^2,$$

and

$$b\,c = \frac{d - e}{a^2}(a - x)^2 + e.$$

Substituting this value in the equation of stress, it became

$$\frac{d^2 y}{d x^2} = \frac{2\,\mathrm{P}}{\mathrm{E} \cdot \mathrm{A}} \cdot \frac{a - x}{\left(\frac{d - e}{a^2} \cdot (a - x)^2 + e\right)^2}$$

Integrating this equation twice, and making y and $\frac{dy}{dx} = 0$ when $x = 0$, the deflection f of the point E, or the value of y when $x = a$, was found to be

$$f = \frac{\mathrm{P}\,a^3}{\mathrm{E} \cdot \mathrm{A} \cdot (d - e)} \cdot \left\{ \frac{1}{\sqrt{e\,(d - e)}} \tan^{-1} \sqrt{\frac{d - e}{e}} - \frac{1}{d} \right\} \quad . \quad . \quad (\mathrm{G}).$$

Taking for the Clevedon pier

$$\begin{aligned} a &= 360 \\ d &= 252 \\ e &= 9 \\ \mathrm{A} &= 8 \\ \mathrm{E} &= 8000 \end{aligned}$$

it was found by this formula that

$$f = \cdot 0765\,\mathrm{P};$$

so that the deflection of the point E by ·0765 inch would, on the above supposition, give an upward reaction against the bottom of the continuous beam at the point E of 1 ton.

Had D C been assumed a straight line instead of a parabolic arc, the deflection would have been ·0195 P, the formula in that case being

$$f = \frac{2\,\mathrm{P}\,a^3}{\mathrm{E}\,\mathrm{A}\,(d - e)^2} \left\{ \frac{d + e}{d} + \frac{2\,e}{d - e} \log \frac{e}{d} \right\}.$$

But the upright of the pier A C B might be regarded as a girder, and the point C would yield sideways by the action of the weight P. Calling h the height A B, and g the deflection at C by a horizontal force F, g might be found by the ordinary formula for a beam supported at the ends and loaded at a point in its length with a weight F.

Hence

$$g = \mathrm{F} \cdot \frac{d^3 (h - d)^2}{\mathrm{E} \cdot \mathrm{I} \cdot 3\,h}.$$

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Taking $h = 720$, $d = 204$, $E = 8000$, and $I = 2064$,

$$g = \cdot 307\ F,$$

or a horizontal force of 1 ton would move the point C through ·307 inch.

By this flexibility of the upright A C B, the point E′ would rise by the action of P, and if another weight P′ were supposed to act at E′, the deflection that would exist there if A C B were perfectly rigid would be modified by the action of P. In like manner, the deflection at E would be modified by the action of P′.

The bracket B C D E being attached to the upright, the horizontal force at C caused by the action of P was $P \cdot \frac{BE}{BC}$. In like manner, the horizontal force caused by the action of P′ was $P' \cdot \frac{BE}{BC}$ in the opposite direction. Hence, calling F the resultant of the two acting together,

$$F = (P - P')\frac{BE}{BC};$$

and the horizontal movement at C in consequence of this was g or ·307 F.

Neglecting for a moment the deflections of the brackets, the movements at E and E′, caused by this movement at C, would be $\cdot 307\ F \frac{BE}{BC}$; and, by substituting the value of F, the movements at E and E′, irrespective of the deflections of the brackets themselves, were

$$\text{for E, } + \cdot 307\ (P - P')\frac{BE^2}{BC^2};$$

$$\text{for E′, } - \cdot 307\ (P - P')\frac{BE^2}{BC^2}.$$

Taking into account the deflections of the brackets, and calling f_1 and f_1' the deflections of E and E′,

$$f_1 = \cdot 0765\ P + \cdot 307\ (P - P')\frac{BE^2}{BC^2}$$

$$f_1' = \cdot 0765\ P' - \cdot 307\ (P - P')\frac{BE^2}{BC^2}.$$

But B E = 30 feet, B C = 21 feet, and, after reduction,

$$f_1 = \cdot 702\ P - \cdot 626\ P'$$
$$f_1' = \cdot 702\ P' - \cdot 626\ P. \qquad \ldots\ (K).$$

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3. Combined action of the piers and continuous beams.

If the continuous beams were supposed to be resting on the piers and brackets and to be afterwards loaded, the deflection of the end of any one of the brackets must be the same as the deflection of the continuous beam at the point where it was in contact with the end of the bracket.

The resistances of the brackets to deflection would cause upward pressures against the continuous beam, and the effect of any one of these pressures on the stresses and deflections of the continuous beam could be ascertained by equations (C), (C″), (D), (D″), bearing in mind that the signs there corresponded to downward pressure, as P was supposed to act downwards. The effect of the whole of the brackets was the sum of the effects of each separately.

Considering the effect of only one bracket, the deflection of the continuous beam at the point in contact with the end of it was the difference between the deflection which would be caused by the load if no bracket existed, ascertained by equations (D), (D′), and the deflection caused by the pressure at the end of the bracket, as ascertained by equations (D), (D″). But the resulting deflection being the same as that of the bracket itself, which might be found by the formula, by putting these two deflections equal to one another, an equation of condition was obtained from which the pressure acting at the end of the bracket could be determined; and from this, the stresses and deflections all along the beam, by forming the equations due to the load, and those due to the now known pressure.

When the effect of all the brackets was considered, the pressure at the end of each bracket would modify the deflection at every point of the continuous beam, and therefore at the ends of all the other brackets. The equation of condition made by equating the deflection of the beam to the deflection of the end of the bracket would therefore contain as many unknown pressures as there were brackets, but since an equation could be formed for the end of each bracket, there were as many equations as unknown quantities.

These equations of condition were simple equations, and by their solution the pressures at the ends of the brackets became known, and from these, by equations (C), (C′), (C″), (D), (D′), (D″), the stresses and deflections at any point of the beam.

Supposing the end span loaded, and that the beam rested on the piers only, the brackets not being in action, the deflections at the

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ends of each of the first five brackets, reckoning from the loaded end of the beam, by equations (D), (D′), were

$$+ 6{\cdot}04, + 5{\cdot}29, - 2{\cdot}54, - 1{\cdot}58, \text{ and } + 0{\cdot}68 \text{ inches.}$$

Now assume that the beam rested on the uprights of the piers and the ends of the brackets, and was loaded on the end span. Reckoning from the loaded end, let z, y, x, v, and t be the unknown downward pressures at the ends of the first five brackets. It was not necessary in this case to consider more than these, as beyond the fifth bracket the action was too small to be worth taking into account. By what had been explained, the following were the five equations of condition, the first corresponding to the end of the first bracket, the second to the end of the second bracket, and so on.

$$\begin{aligned}
&+ 6{\cdot}04 - {\cdot}1845\,z - {\cdot}1275\,y + {\cdot}0555\,x + {\cdot}0332\,v - {\cdot}0125\,t = {\cdot}0765\,z \\
&+ 5{\cdot}29 - {\cdot}1275\,z - {\cdot}1443\,y + {\cdot}0727\,x + {\cdot}0441\,v - {\cdot}0180\,t = {\cdot}702\,y - {\cdot}626\,x \\
&- 2{\cdot}54 + {\cdot}0555\,z + {\cdot}0725\,y - {\cdot}1246\,x - {\cdot}0914\,v + {\cdot}0394\,t = {\cdot}702\,x - {\cdot}626\,y \\
&- 1{\cdot}585 + {\cdot}0347\,z + {\cdot}0453\,y - {\cdot}0739\,x - {\cdot}1215\,v + {\cdot}0625\,t = {\cdot}702\,v - {\cdot}626\,t \\
&+ 0{\cdot}679 - {\cdot}0148\,z - {\cdot}0194\,y + {\cdot}0400\,x + {\cdot}0628\,v - {\cdot}1201\,t = {\cdot}702\,t - {\cdot}626\,v
\end{aligned}$$

The first terms on the first side of these equations were the deflections given above for the continuous beam if rested only on the uprights of the piers. Any one of the other terms on this side of the equations, as for instance $+ {\cdot}0725\,y$ in the third equation, expressed the deflection in inches of the continuous beam at the point corresponding to the end of the third bracket by the action of the force y as found by equations (D), (D″).

The second sides of the equations were the deflections of the ends of the brackets by formulæ (G) and (K).

It was not difficult to find a sufficiently approximate solution of these equations. For this purpose, assume y, x, v, and t each $= 0$. Then

$$z = 23{\cdot}14 \text{ tons.}$$

Next assume only v and t each $= 0$, and using this approximate value of z, it was found that

$$\begin{aligned}
z &= 21{\cdot}05 \text{ tons.} \\
y &= 5{\cdot}64 \quad ,, \\
x &= 3{\cdot}11 \quad ,,
\end{aligned}$$

By using these values as approximations, it was soon found by a few trials that a sufficiently accurate solution was

$$\begin{aligned}
z &= + 20{\cdot}8 \text{ tons.} \\
y &= + 5{\cdot}6 \quad ,, \\
x &= + 2{\cdot}9 \quad ,, \\
v &= - 2{\cdot}0 \quad ,, \\
t &= - 1{\cdot}2 \quad ,,
\end{aligned}$$

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By means of these values and that of Q the load, which was taken at 50 tons, the values of r_0, r_1, r_2, &c., could be determined, and from them the equations of stress and deflection.

The curves corresponding to the end span loaded were shown at the right-hand side of Fig. 2, Plate 12A.

If all the spans were loaded uniformly, each with the weight Q, and the continuous beam rested on the piers, the brackets being supposed removed, the deflections at the points corresponding to the ends of each of the brackets, reckoning from the end of the beam, were by equations (F),

4·69, 3·45, 0·63, 0·93, 1·69, 1·61, 1·47, and 1·58 inches.

Now if the beam were supposed to be rested on the uprights of the piers and ends of the brackets before being loaded, each bracket would take a share of the load, and the action of none of them could be neglected. Reckoning from the end of the beam, let $z, y \ldots q$ be the unknown downward pressures at the ends of each of the brackets up to the middle of the beam. There would be a corresponding and equal set of pressures for the brackets on the other half of the beam, and these must be taken into account in forming the equations of condition.

In this manner the following equations were obtained, where, as before, the first corresponded to the end of the first bracket, the second to the end of the second bracket, and so on.

$$4{\cdot}69 - {\cdot}1845\,z - {\cdot}1275\,y + {\cdot}0555\,x + {\cdot}0332\,v - {\cdot}0125\,t - {\cdot}0130\,s + {\cdot}0003\,r + {\cdot}0001\,q = {\cdot}0765\,z$$
$$3{\cdot}45 - {\cdot}1275\,z - {\cdot}1443\,y + {\cdot}0727\,x + {\cdot}0441\,v - {\cdot}0180\,t - {\cdot}0135\,s + {\cdot}0018\,r + {\cdot}0034\,q = {\cdot}702\,y - {\cdot}626\,x$$
$$0{\cdot}63 + {\cdot}0555\,z + {\cdot}0725\,y - {\cdot}1246\,x - {\cdot}0914\,v + {\cdot}0394\,t + {\cdot}0251\,s - {\cdot}0067\,r - {\cdot}0027\,q = {\cdot}702\,x - {\cdot}626\,y$$
$$0{\cdot}93 + {\cdot}0347\,z + {\cdot}0453\,y - {\cdot}0915\,x - {\cdot}1215\,v + {\cdot}0625\,t + {\cdot}0395\,s - {\cdot}0128\,r - {\cdot}0044\,q = {\cdot}702\,v - {\cdot}626\,t$$
$$1{\cdot}69 - {\cdot}0148\,z - {\cdot}0194\,y + {\cdot}0400\,x + {\cdot}0628\,v - {\cdot}1201\,t - {\cdot}0863\,s + {\cdot}0312\,r + {\cdot}0124\,q = {\cdot}702\,t - {\cdot}626\,v$$
$$1{\cdot}61 - {\cdot}0093\,z - {\cdot}0215\,y + {\cdot}0244\,x + {\cdot}0383\,v - {\cdot}0848\,t - {\cdot}1128\,s + {\cdot}0442\,r + {\cdot}0094\,q = {\cdot}702\,s - {\cdot}626\,r$$
$$1{\cdot}47 + {\cdot}0010\,z + {\cdot}0053\,y - {\cdot}0110\,x - {\cdot}0145\,v + {\cdot}0329\,t + {\cdot}0535\,s - {\cdot}0974\,r - {\cdot}0560\,q = {\cdot}702\,r - {\cdot}626\,s$$
$$1{\cdot}58 + {\cdot}0024\,z + {\cdot}0032\,y - {\cdot}0067\,x - {\cdot}0060\,v + {\cdot}0139\,t + {\cdot}0220\,s - {\cdot}0494\,r - {\cdot}0575\,q = {\cdot}702\,q - {\cdot}626\,q$$

The explanation of the terms of these equations was the same as that given above for the beam loaded on the end span only; but it was to be remarked that, for instance, the term $+ {\cdot}0329\,t$ in the seventh equation, which expressed the deflection of the beam at the end of the seventh bracket, was the sum of two terms, one, $+ {\cdot}0394\,t$, for the action of the bracket on the right-hand side of the centre of the beam, and another, $- {\cdot}0065\,t$, for the action of a corresponding bracket on the left-hand side of the centre, and so on for the other terms.

Proceeding with the solution as in the former case, the successive approximate solutions were

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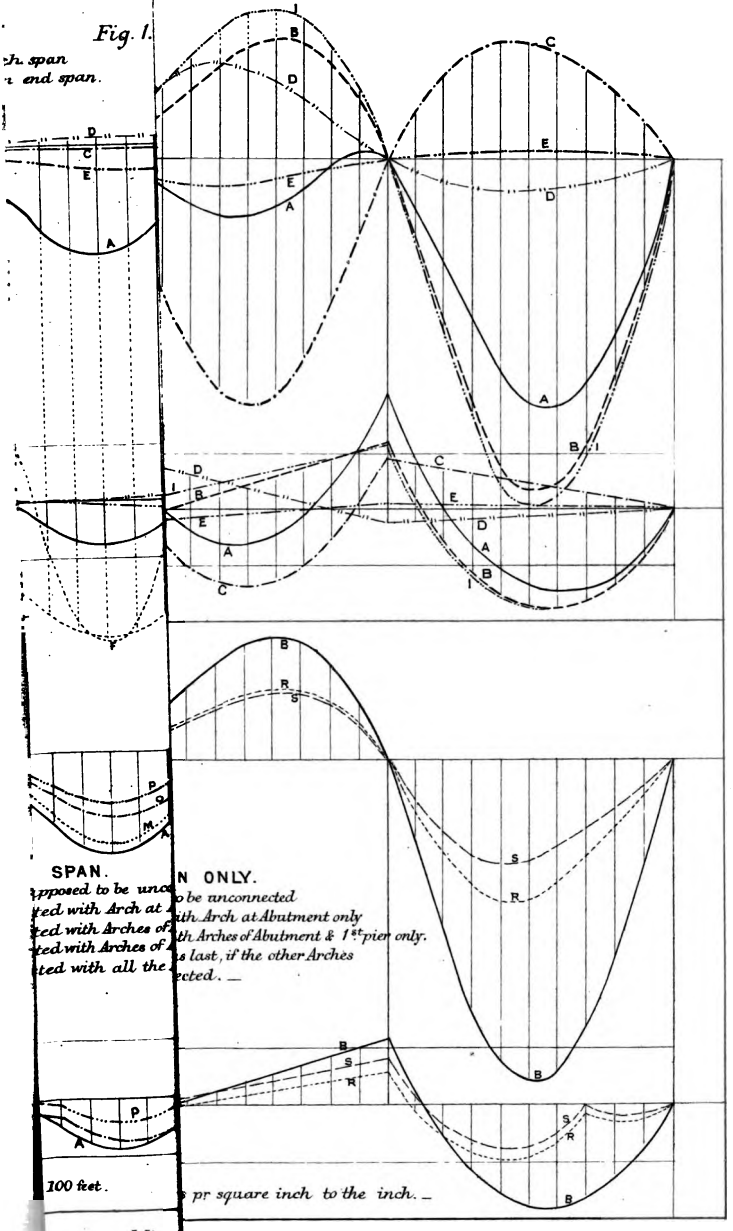

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z = 14·4, 15·1, 16·2, 15·7
y = 10·8, 9·6, 9·5
x = 10·9, 9·3, 9·2
v = 10·7, 9·9
t = 11·0, 9·9
s = 9·1
r = 9·2
q = 10·5

For these last values of z, y, x, &c., the residual errors of the several equations were each less than ·1, or the elastic curve was determined within a probable error of ·1 inch. Using these values, r_0, r_1, r_2, &c., were determined, and from them the equations of stress and deflection.

The curves calculated in this manner were shown on the left-hand side of Fig. 2, Plate 12A.

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April 4, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

THE following Candidates were balloted for and duly elected:—ROBERT LUNN, JOSEPH WILLIAM TRUTCH, and HENRY WOOD, as Members; HENRY BRADY, EDWARD BUCKHAM, ARCHIBALD DAVIS DAWNAY, SYDNEY GEDGE, M.A., GEORGE WILLIAM GOODISON, JOHN JACKSON, CHRISTIAN HENDRIKS MEŸER, EDWIN MUIR, HOWARD DEVENISH PEARSALL, Stud. Inst. C.E., THOMAS MELLARD READE, CAMPBELL THOMSON, JOHN THOMSON, ROBERT VAWSER, JAMES HENRY WHITTLE, and WILLIAM YUILL, as Associates.

It was announced that the Council, acting under the provisions of Section III. Cl. VII. of the Bye-Laws, had transferred BENJAMIN CHAPMAN BROWNE, GEORGE EEDES EACHUS, and ALGERNON JOY, from the class of Associate to that of Member.

Also that under the provisions of Sect. IV. of the Bye-Laws, ALFRED HENRY HOLLIS had been admitted a Student of the Institution.

No. 1,281.—"On the Testing of Rails; with a description of a machine for the purpose." By JAMES PRICE, M. Inst. C.E.

THE best mode of determining beforehand the fitness of rails for their work is a question of the greatest interest, especially to those engineers who have been in practice long enough to see the end of both good rails and bad rails. The disadvantages of the latter may be stated under three heads:—The danger arising from sudden breakages, and from worn rails before they can be replaced. The danger in replacement, for where so much depends on human skill and care, failure must sometimes take place; besides, the mere laying down of the new rails, and the removal of the old, are attended with risk and inconvenience to the traffic. Lastly, the economic question, involving great expense in labour and materials, including not only rails, but sleepers and fastenings, which, if left alone, might have lasted many years longer. Such remarks would be hardly necessary, except that engineers in constructing railways often disregard these considera-

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tions, and, to effect a trifling saving in first cost, admit elements of future weakness.

In addition to the ordinary and unavoidable causes whereby rails are worn out, there are other causes of wear which are avoidable. Of these, two are often overlooked, but they are, nevertheless, of consequence.

First, where on the same system there are different sections of rail, it frequently happens that the forms of the rail-tops vary very much in width and roundness; the consequence is that the tires of those engines which are worked mostly on one branch become worn to suit that particular rail-top; and when it is necessary to run such engines over the other parts of the railway, the tires do not fit the rails, but roll along edges, not surfaces (Plate 13, Fig. 1). In this manner the rails are gradually broken down, and much injured. Care should therefore be taken that, on all extensions and branch lines, the rail-tops at least should conform to those on the parent line. On the whole, a moderately flat-topped rail is to be preferred to one much rounded.

Secondly, where a railway abounds in curves, there is considerable increase of wear if the super-elevation of the outer rail is not properly attended to; it is often too great, and the inner rail suffers accordingly. The difficulty in instructing platelayers and inspectors on this point arises from the fact, that the super-elevation is given in terms of the radius, which is not generally known, and which must be ascertained by the use of another formula. The Author has prepared a simple rule, which is theoretically true, and requires no calculation. The term 'radius' is eliminated, and a chord is found, the versed sine of which is equal to the proper super-elevation. The length of this chord is the same for curves of any radii, and depends solely on the assumed speed and the gauge, which are practically invariable. For a speed of 40 miles per hour, this chord will be 64 feet in length for the English gauge, and 67 feet for the Irish gauge (5 feet 3 inches); so that a chain length (66 feet) is sufficiently near for both.

The rule is as follows:—Stretch a line between two points one chain apart on the hollow side of a single series of rails; the distance from the middle point of this line to the edge of the rail is the correct super-elevation (Fig. 2). For any gauge and any velocity the formula becomes

$$\text{Length of chord} = \tfrac{1}{2} \text{ velocity in feet per second } \sqrt{\text{gauge}}.$$

The proof of which is as follows:

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Let y = gauge.
g = force of gravity = 32.
v = velocity in feet per second.
r = radius of curve.
l = length of chord required.
d = versed sine.
And x = super-elevation.

Problem: find l, so that $x = d$. That equilibrium may exist, the centrifugal force must equal the resistance of the inclined plane, or

$$\frac{v^2}{g\,r} = \frac{x}{y}, \text{ therefore } x = \frac{y\,v^2}{g\,r},$$

but $$r = \frac{l^2}{8\,d}, \text{ therefore } x = \frac{8\,y\,v^2}{g\,l^2} \times d;$$

in order that (x) may equal (d),

put $$\frac{8\,y\,v^2}{g\,l^2} = 1;$$

inserting the value of (g),

$$l^2 = \frac{8\,y\,v^2}{32}, \text{ therefore } l = \frac{1}{2}\,v\,\sqrt{y}.$$

The Author has used a simple level for ascertaining the heights of the rails; it consists of a thin iron gauge, with a hole near each end, and a piece of string about 7 feet long; the ends of the string are looped through the holes, and a knot adjusted in the middle, so that the bar will hang level over still water if suspended from the knot held freely between the finger and the thumb (Fig. 3).

The qualities sought in a rail are fourfold:—First, strength, as a girder, to sustain a moving load; secondly, toughness, to resist sudden strain or impact; thirdly, solidity, to resist separation under pressure; fourthly, hardness, to resist wear of the surface. The first and second affect the safety of a road, and are therefore more important than the third and fourth, which are economic properties regulating the cost of renewals and the amount of available profits.

1st. Strength.—This quality depends both on the form of the rail, and on the nature of the iron. The double-headed, and the flange, or Vignoles rails, are all that can be desired as to form for strength, though the latter has somewhat the advantage, the iron in the bottom flange being disposed at a greater distance from the neutral axis, besides being a better form to resist side strains and torsion.

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Rails of the bridge type are bad girders; iron is wasted in the double vertical web at the expense of the bottom flange; such rails possess hardly two-thirds the strength of rails equal in weight, but of girder forms. In the Author's experience of bridge-rails, whether on longitudinal or on cross sleepers, about 30 per cent. break through the flange holes long before the tops are worn out. Being at the same time stiff and weak, they are unsuited to the elasticity of longitudinal sleepers, and are unfit for a cross-sleeper road.

The consideration of the strain which a rail has to bear as a girder naturally suggests an inquiry into the proper interval between the sleepers. This is a question of importance, and capable of easy solution. Experience will show the amount of base area of sleepers required to sustain a given weight of traffic over a known substratum; but experience will not necessarily indicate the best arrangement of this bearing surface,—that is, the correct spacing of the sleepers. When the driving-wheel of an engine is placed exactly over a sleeper, the surface of the rail is depressed, owing to the compression of the timber and the yielding of the ballast. Let this depression = a. When the wheel stands over the centre of an interval, the weight being borne equally by two sleepers, the depression over each becomes less. Let its amount = b. Then, in order that the line of motion may be straight, and not undulating, the deflection of the rail must = $a - b$ (Fig. 4): if it is greater, there will be hollows between the sleepers; if less, hollows over the sleepers.

To carry this idea into practice, the Author fixed indices in different positions on the sides of rails within view of a level, and, placing the driving-wheel of an engine on the above-mentioned parts, found that with the sleepers 3 feet apart, from centre to centre, a flange rail, weighing 80 lbs. to the yard (Fig. 5), did not deflect enough, so that the centre rose 0·03 inch; but that with a bridge-rail of the same weight (Fig. 6) the deflection was too great, as the rail sunk in the centre 0·1 inch. Though the amount of deviation from a straight line is minute, still from its rapidity the effect is sensible. Assuming a speed of 40 miles an hour, there would be thirty-eight vibrations per second, which must have an injurious effect both on rails and wheels. The spacing of the sleepers should therefore be increased or diminished as may be necessary; the rule being to vary the spacing when rails of different degrees of stiffness are used, so that the cubes of the clear intervals may be proportional to the stiffness.

The Author has adopted an interval of 3 feet 6 inches, from

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centre to centre, in preference to one of 3 feet for a high flange rail (Fig. 7); and by employing larger sleepers, so as to have the required bearing, more massive and better grown timber is obtained without any increase of cost.

The interval between the sleepers being fixed upon, the testing span can be arrived at in this manner. A rail to be thoroughly safe should not break, or take a permanent set under the weight of an engine, even if every alternate sleeper were left out of the road; for in practice any one sleeper may be so badly packed, or decayed, as to offer no resistance to a deflection of $\frac{3}{8}$ths of an inch, which is about the extent to which a good rail will bend under an engine when spanning an interval of 6 feet. The reason for not taking a case of two bad sleepers together is, that the rail under such circumstances would deflect so much that it must find some support before it could break. It would be disastrous if a single case of bad packing should lead to the breakage of a rail; therefore, it may be assumed that all rails should be tested for strength at such a clear interval as would be obtained in practice, by leaving out every alternate sleeper. The Author has adopted for this purpose a span of 6 feet between the supports.

2nd. Toughness.—This quality is as necessary as strength. It is possible for a rail to bear a great dead weight, and yet to break easily under a suddenly applied strain. The latter being the condition of strain to which rails are subjected, toughness becomes essential. It is best measured by the extent to which the iron composing the rail will stretch before fracture is produced by direct tension. The Author obtained from 8 to 9 per cent. of elongation in tearing asunder strips cut from the base of flange-rails, which strips broke under a direct tensile strain of 21 tons per square inch of original section. One of the many advantages possessed by flange-rails is the opportunity this form affords of having a tough fibrous iron in the bottom, and material of a hard compact quality in the head. As a matter of practice, none but ductile tough iron can be rolled into a wide flange; consequently, it is found that flange rails never break in the road.

3rd. Solidity.—Of the economic qualities this is the most important; by which is meant freedom from particles of foreign matter, and perfect welding. The rapid lamination, breaking down, and destruction of rails, is mainly due to the absence of this property.

4th. Hardness, though desirable, is not so essential as the other qualities. A rail that is of solid texture will rarely be so soft as to lose its form from bulging; the top will even harden under the

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cold rolling to which it is subjected. At shunting stations, however, and on inclines, under the influence of sand and breaks, the surface of a soft rail is rapidly worn away.

Existing modes of testing rails may be divided into dead-weight tests, falling-weight tests, and examination of fracture.

The dead-weight tests determine the strength of a rail as a girder; but in practice, rails are subjected to strains of a different character, namely, that produced by rolling weights; the results are, therefore, not reliable.

The endeavour to ascertain whether a rail has sufficient toughness, by submitting it to the action of a falling weight, is most fallacious. This test goes as far beyond the mark as that by dead weight falls short of it; because it requires an amount of toughness which is quite unnecessary, and incompatible with a proper degree of hardness. By its use rails of a very serviceable quality may be rejected, while bad rails escape. The unfairness of this mode can be shown by considering the effect of a weight falling, say 4 feet, which is considered a low fall, with the action of a rolling weight advancing at the rate of 40 miles per hour. The falling weight strikes and bends the rail with a velocity of 16 feet per second, whereas the rolling weight, the interval being 6 feet and the deflection $\frac{3}{8}$ths of an inch, bends the rail with a velocity of $7\frac{1}{4}$ inches per second, or $\frac{1}{26}$th part of the former velocity.

The determination of the solidity of a rail is hardly attempted. Appearance of fracture, or a planed section treated with acid, may to a practised eye be a criterion, but it is not sufficiently tangible to embody in a specification.

The deflection of a rail under a given load is generally assumed to be inversely as its hardness, and is therefore considered a measure of the absence of this quality. However this may be, it is not applicable to flange-rails, which, if properly made, with hard tops and tough flanges, will deflect considerably; and in a double-headed rail it is difficult to conceive how hardness, as shown by a small deflection, can co-exist with extreme toughness to resist the impact of a falling weight; such iron may be procured, but not among those qualities used for rails.

From the foregoing and other reasons, the Author, having arrived at the conclusion that existing tests bear no analogy to actual work, designed a machine for the purpose of subjecting rails to the test of rolling weights, whereby both their strength and their wearing properties could be determined with accuracy.

The machine consists of a simple arrangement of two or more

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metal or steel rollers, supporting a circular frame which confines them. The frame is connected by radii with a centre boss, through which passes a vertical axle (Fig. 8); or the frame may be cruciform for four rollers (Fig. 9), or a simple beam passing across the centre for two rollers (Fig. 10): in every case sufficient strength being given to the radii, or arms, to resist the centrifugal force generated when revolving. The rails to be tested are formed into a circle or polygon, and are supported on sleepers, packed up with ballast in the usual manner. This arrangement forms the road under the rollers, upon which they are caused to travel at any required speed, by means of motion communicated to the centre axle from shafting underneath, or by direct action of a pair of cylinders. The frame is sufficiently stiff to bear entirely on the rollers; and the boss should be free to move up and down on the vertical axle, so that the machine may conform itself freely to the inequalities of the road.

The weight with which each roller bears upon the rails should be equal to the weight of the driving-wheel of the heaviest engine likely to be worked over the same rails when in actual use. The rollers should have treads sufficiently wide to work over the rails arranged as a polygon (Fig. 11), and being portions of cones radiating to the centre at the level of the top of the rails (Fig. 12), they will traverse over a polygon as easily as along a curve.

In using the machine to test the strength of rails, some of the sleepers are left out at several parts of the circle, so that the rails at those places may span double intervals. If a rail takes a set or breaks under the action of the revolving rollers it is unfit for use, for the reasons before mentioned. To carry the experiment farther, so as to arrive at the proper margin of strength, the lower member is weakened, by cutting or drilling, until it breaks. By treating rails of known good quality in a similar way, a standard of strength can be obtained.

The determination of the wearing properties of rails is manifestly accomplished by keeping the machine revolving until the rails are worn out.

Before commencing the series of experiments detailed hereafter, the Author assumed the substantial truth of that law according to which rails are worn out by the action of a certain number of 'speed-tons,' which was first demonstrated by Mr. Price Williams, M. Inst. C.E., from a collection of facts obtained on the principal railways.[1] This law, however, did not separate the action of the lighter

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxv., p. 353.

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traffic of wagons and carriages from the wear produced by engines; so that it could not be determined *a priori* how many 'speed-tons' rails would bear if subjected to the action of this machine, which is analogous to the continuous passage of trains of engines. It was, however, hoped, and partly expected, that the results would be much more rapid under the machine, so that the test might be easily carried out. This has proved to be the case, which demonstrates, what might have been anticipated, that the number of 'speed-tons,' borne by any rail during its life under ordinary traffic, must be reduced by some function of the intensity of the weights at their applied points. It is almost self-evident that weights applied in small parcels, say of a few pounds, would never cause rails to laminate or crush; whereas, so great a weight might be applied at a single point that the rail would be utterly destroyed at once.

The absence of springs will no doubt add to the severity of the test; but if this is made an objection, there is no difficulty in using springs by lightening the rollers, and throwing the greater part of the weight into the frame. Such, however, might not be desirable, as the prolongation of the test is unnecessary, a ratio of comparison with a standard rail of known good quality being all that is sought for. It will be admitted if ten rails are placed under the machine together, one being a standard, that those that wear out before the standard are inferior to it; that those which wear out first are the worst; and that the number of 'speed-tons' borne by each is the measure of its value.

Objections on the score of the texture of the iron having been injured by bending have, in the Author's experience, been found of no moment, provided the rails are bent at a dull red heat. But, as before remarked, the rails can be used straight, arranged as a polygon, which plan possesses the collateral advantage that the rollers wear more evenly over their entire surface.

The wearing effect produced by a machine having four rollers bearing with a pressure of 7 tons each, upon a ring of rails 40 feet in diameter, and worked at a speed of 40 miles per hour, may be stated thus: 42 revolutions per mile $\times$ 40 miles per hour $\times$ 22 hours $\times$ 28 tons = 1,034,880 tons per day of 22 hours, allowing 2 hours out of every 24 hours for oiling, packing, &c.; this would give 41,395,200 speed-tons per day. Two such days' work would discover any defective rails, if not destroy the best.

The machine constructed by the Author, from which the results hereafter detailed have been obtained, consists of a horizontal beam supported on a pair of metal rollers, 5 feet in mean diameter and

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16 inches wide. The circle traversed is 40 feet in diameter. The rollers weigh 2¼ tons each, and with the beam weighing 6½ tons, the pair of rollers press on the rails with a total weight of 11 tons. One roller bears with 5 tons' pressure, and the other with 6 tons. Motion is imparted to the centre vertical axle by means of shafting underneath, and bevilled wheel and pinion, driven by a steam-engine. The power is sufficient to get up a speed of 20 miles per hour. As the engine works a sawmill during the daytime, it has been necessary to use the machine mostly by night. A counter is employed to register the revolutions. The experiments first tried by the Author had reference to the strength of rails, when it was ascertained that if the rails were curved while hot no diminution of strength took place. In the next place, a flange rail, of the section shown in Fig. 13, weighing 76 lbs. to the yard, after having been in use on a railway for seven years without showing any flaw, during which time traffic amounting to about 3,500,000 tons passed over it, when placed in the circle and left unsupported for a clear interval of 6 feet did not break or take a set. The flange was then drilled in the centre of the span with two ¾-inch holes; in this instance it did not break or take a set under the rollers travelling at the rate of 16 miles per hour. Again the flanges were cut away from the holes to the outer edges (Fig. 14); when traversed by the rollers it took a set but did not break, though no doubt it would have done so under long-continued rolling. Finally, the flange was weakened by cutting it away, as shown shaded on Fig. 15, when it at last broke. This rail possessed a wide margin of strength, and having a hard compact top, as subsequently proved under the wearing process, must be reckoned as one of first-class quality. Other rails of like section were broken, and the results obtained were not very different.

The next experiments had reference to the wearing properties of rails. Eight samples of foot rails were tested, and six of these have been worn out. The rails were manufactured at different times by five different firms. The results are given in the Appendix.

The third set of experiments referred to the strength and toughness of Bessemer steel rails. As their wearing properties were undeniable, attention was only directed to their bearing qualities. No doubt some few out of almost any large number of Bessemer steel rails will be found too brittle for use; and any one of these breaking in the road and causing an accident would bring discredit on the whole. It is therefore desirable that a very large number, if not every rail, should be placed under the machine for a few

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revolutions, with a clear interval of 6 feet between the bearings. This can be effected in a simple manner, as shown in Fig. 16. Each rail across the gap is subjected to fifty revolutions, and then withdrawn endways and another pushed into its place, without stopping the machine; at a single opening 4 or 5 tons of rails per hour can be thus tested. In this manner the Author tested four hundred and twenty-seven rails of Bessemer steel, of the section shown in Fig. 7, out of which only four broke; any one of these, however, would have broken under ordinary traffic.

The Author has not, as yet, made a sufficient number of experiments on the breaking of steel rails to arrive at the true characteristics of that material. However, the following experiment seems to show, that steel and iron are very differently acted on by rolling weights. A steel rail of the section shown in Fig. 17, weighing 80 lbs. to the yard, having been put under the rollers, broke when weakened in the flange to the extent shown shaded; whereas the iron rail before mentioned, and weighing only 76 lbs. per yard, required a greater amount of weakening. Strips having been cut from the flanges of both of these rails and subjected to direct tensile strain, the steel bore 30 tons per square inch before fracture, and stretched 20 per cent. of its length, and was then so tough as to bend double cold; but the iron bore only 21 tons per square inch, and stretched only 9 per cent. This indicates that steel, though stronger and apparently tougher under slowly applied strain, is really more brittle than iron under sudden strain, as if the molecules of steel required time to arrange themselves to resist separation.

The effect on the surface of the metal rollers from long-continued work over a circular ring is very considerable after the outer skin has worn off. The Author is preparing to have the rollers turned down and fitted with steel tires, which was always contemplated. When worked over polygons, to bring the wear evenly over the whole surface, each set of tires will last a long time.

It may be remarked, in conclusion, that this machine will also test the strength of all fastenings, sleepers, and ballast. Almost all the ordinary type of fish-plates break. Those with a small flange below, as shown in Fig. 18, alone stand under the machine. The rails also show a weakness at the fish-holes, which should be abolished, if practicable.

The effect produced on rails by breaks, sand, or water, can also be ascertained.

As to the probability of rails heating under a more destructive machine than the Author's, it may be remarked that all the heat

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generated will be as rapidly conducted and radiated away by the large mass of iron in the rails and rollers, so that an accumulation will be impossible. In planing down a rail more rapid destruction takes place, more heat is therefore generated, but the rail does not heat.

Such machines established in proximity to the centres of iron and steel manufactures would be beneficial in satisfying Engineers and railway companies as to the quality of the materials they were procuring, without the present slow and unpleasant process of paying for their experience.

The communication is accompanied by a series of diagrams, from which Plate 13 has been compiled.

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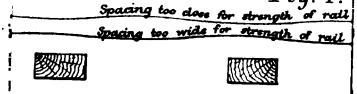
TESTING

Fig. 4.



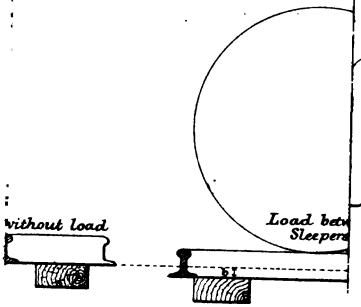


Fig. 6.

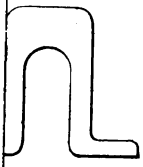


Fig. 7.

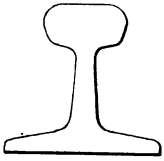


Fig. 10

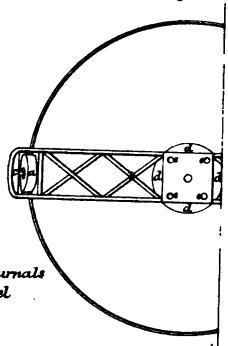


Fig. 16.

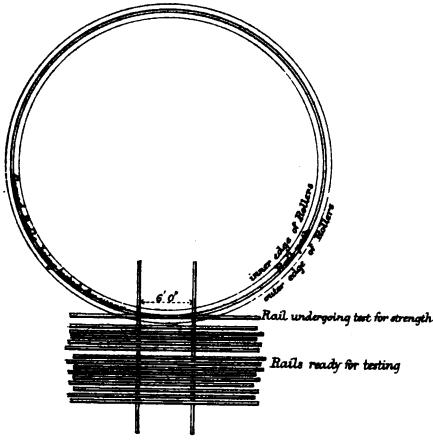


Fig. 14.

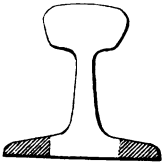
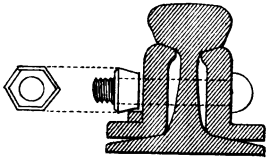


Fig. 18.



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APPENDIX.

TABLE of the RESULTS of EXPERIMENTS with FOOT RAILS.

Mark.	Weight per Yard.	Number of Tons passed over.	Speed per Hour.	Speed-Tons.	Remarks.
	lbs.		miles.		
A	76	3,355,482	14	46,976,748	This rail was in the railway under ordinary traffic for 7 years previously, and must have borne 3½ million tons at 20 miles per hour; it is not yet worn out, but broken at the fish-holes.
B	76	3,964,511	13	51,538,643	Two new rails, not yet worn out.
C	76	1,861,277	14	26,057,878	This rail worn out.
D	76	1,861,277	14	26,057,878	This rail was bent cold; worn out.
E	80	3,510,540	13	45,637,020	This rail worn out in one part.
F	68	1,098,361	14	15,377,054	This rail worn out.
G	68	834,548	14	11,683,672	This rail worn out.
H	60	416,240	10	4,162,400	Worn out. Two rails, one straight and other curved.

[Mr. J. PRICE

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Mr. J. PRICE observed that the extreme difficulty in obtaining good rails, no matter how carefully the specification was drawn up, had led him to the consideration of a machine for the purpose of testing rails. He had found that rails when from the same maker and of the same price were often very different in quality. He did not say that was the fault of the manufacturers, but he thought they required to be instructed how to apply a test whereby it could be ascertained what was the most serviceable quality of iron for rails and what was the best mode of manufacturing them. He had been struck with the difficulty of getting rails equal in durability to those laid down twenty years ago, some of which were still in use, whilst rails which had not been laid down half that time were nearly or wholly worn out.

He had not stated in the Paper the number of rails which he considered it necessary to test; but practically he had adopted one in two hundred, and he thought as a rule any greater number than that was unnecessary. In selecting at random five rails out of one thousand for the purpose of testing, it was most improbable that those five rails should belong to those few of exceptionally bad quality which might be found in any large lot of really good rails; therefore the chances in favour of the test being a true indication of the quality of the lot were greater than the chances that the only bad rails had been selected for trial.

With respect to the connection of the frame which bore on the rollers with the driving rays attached to the bevel-wheel, he had arranged that the ends of the rays should merely go into a notch in the frame, so that the latter should be free to move vertically, and not affect the wheel. In the first experiments he made, those parts were rigidly connected together, which caused the teeth of the wheel to break out; but since he had disconnected them and left them to move freely up and down none of the teeth of the wheel had been broken out.

He believed the destruction of rails from the wheel tires of engines not fitting the rails properly was greater than was generally considered. He had found that when the rollers of the machine were worn and cut edgeways, and gave a side-rubbing to the rails, the wear of the latter was enormous; and that would account for the great wear of the inner rails on a curve if the superelevation of the outer rail was too great; or, what was the same thing, if the speed of the carriages was less than the superelevation was calculated for, in which case a continual sliding motion took place, tending to wear out the inner rail of the curve.

He wished to call attention to another fact. The experiments

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proved that it was the engines that really destroyed the rails. The lighter traffic of the wagons and carriages was comparatively harmless; though the same number of tons passed over they did not wear out the rails as the engines did. On that account he suggested that the immense weight put upon the driving-wheels of the locomotive should be spread over more points, by making as many wheels to act as drivers as possible.

With regard to steel rails he found it expedient to test every rail; the time occupied was very short, as the rails could be tested without stopping the machine to change them: as fast as one rail was removed another could be pushed in; and in that way between 4 and 5 tons of rails per hour could be proved.

Mr. BRAMWELL asked whether, where the bottom flanges of the rails were cut away for the purpose of diminishing their strength under trial, the cutting away was done to any considerable length or was merely a notch; and if of considerable length, whether the ends of the cuttings were made at right angles, or were gradually curved off? He made this inquiry, because the effect in weakening the rail would be different according as the cutting away was made locally, or if it were made with a gradual termination.

Mr. PRICE replied that the cut in the flange was local; it was meant to be at right angles to the axis of the rail, and the edges were left round. When the cut was made the rail took a permanent set, and he considered that the rail was destroyed. The notch had a circular termination, and the cut was about $\frac{1}{2}$ inch wide.

Mr. BRAMWELL observed that a steel rail, having been in fusion, was of uniform texture, and was united throughout; but an iron rail was an aggregation of bars piled and rolled. The rolling caused metallic union more or less, and this union was supplemented by a cementing together of the surfaces due to the foreign matter between the bars. These were the differences of structure between a steel and an iron rail; and he thought with these differences it might well be the case, that a steel rail cut with a narrow notch would suffer more than an iron rail cut with a similar notch. He had been for some years considering the subject of the influence of an abrupt change of form upon the strength of steel and iron, and he had endeavoured to show why an abrupt change made the small part in the neighbourhood of the large part not merely relatively, but also actually weaker. Now, the reasons for this fact applied with much greater force in the case of a homogeneous material, like that of a steel rail, which had been in fusion, than they did in the case of an iron rail built up of a number of comparatively ill-combined bars; and therefore he could understand that, though the steel in itself was tougher and stronger, when

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tested separately, than iron, yet that it would be weaker under the circumstance of the abrupt cutting than the iron would be. He presumed that, when Mr. Price Williams took his test in 'speed tons,' the tons included both the weight of the trains and of the engines. If so, and if the Author had reproduced in his testing the tons entirely in the shape of heavy engine pressure, having nothing to represent the light rail pressure, then such a test would be necessarily more severe than that of actual trains of wagons and carriages. But there was one point in which he thought this machine was deficient, as not representing an element of wear in real work. It was this:—When an engine went over rails, it not only injured them by its weight, but also by making use of the rails as a means of driving itself and the train along. In the case of this testing machine, the rollers were driven over the rails by gearing; there was no effort exerted by the rollers to turn the machine in the way that an engine exerted effort to drive itself ahead; and in that respect he should think the test was less severe than the passage of an engine. Furthermore, there were not the oscillations and side shocks in this machine that there were in a train; nevertheless it was an apparatus which gave comparative results, and had evidently been well thought over and well worked out.

The Author's mode of arriving at the proper elevation of the outer rail on a curve was one that could be carried out by ordinary workmen without difficulty. He had thought his own rule was a simple one, viz., $\frac{1}{16}$ of an inch elevation for 1 chain radius, and 1 mile per hour; as being all in units it was easily remembered, and the common variation inversely as the radius, and directly as the square of the velocity, was easily applied; but this required the radius to be known. The plan of the Author, which arrived at the same results as his own, was infinitely better, because it required no knowledge of the radius; and from the fact of the radius being eliminated the operation of finding the elevation of the outer rail was brought at once within the grasp of any ordinary workman.

Mr. C. W. SIEMENS said that having lately made some experiments with steel rails and iron rails, he could bear out the Author's remarks, confirmed already by Mr. Bramwell, as to the great susceptibility of steel to break under the influence of a shock. The Paper which Mr. Bramwell read before the British Association[1] pointed out the reasons why it was so. In the case of an iron rail there were innumerable fibres capable of yielding individually to take a greater strain when necessary. Now in the case of steel which

[1] *Vide* Report of British Association, 1859, p. 422.

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was homogeneous, no such independent fibrous action could take place, and if one part of the line of strength was broken an indefinite amount of tearing strain was thrown upon the adjoining part. In trying steel rails under a blow, he found that where an entire rail would stand the impact of a ton weight falling 20 feet, in the case of a rail being supported rigidly between 3-feet bearings, a blow from a fall of only 4 feet of the same weight would break it at the place where the hole was drilled through the web of the rail. It followed from this experiment that in drilling or punching a number of holes through a rail the strength of that rail was diminished to less than one-fourth, as measured by a falling weight; and this fact should lead to the preference being given to a mode of fastening in which no such holes were made. It was true that in ordinary use no such working strain as the weight of a ton falling through a number of feet was put upon a rail; but Engineers liked to err upon the right side in a question of safety, and were justified, he thought, in giving the preference to steel rails of mild or ductile quality; for if the steel was not of tough quality, there was danger of its being hardened by accidental exposure to cold during manufacture. Steel containing more than $\frac{3}{10}$ per cent. of carbon would become very brittle if by accident it was thrown into water when red hot; and such a rail might break with a very slight shock. In France it was the habit to make steel rails containing $\frac{8}{10}$ per cent. of carbon, and as far as wearing qualities were concerned French engineers were right; but there was some danger in using such rails, particularly if the continuity of the rail was broken by holes. Therefore he did not agree with the Author in his assertion that the test by blows was altogether inadmissible: he thought it a desirable test to apply along with others. The particular test which the Author proposed was valuable in so far as it proved the fair wearing quality of the material; but he did not consider it sufficient as a test for the wearing quality of rails under the most trying circumstances. Mr. Bramwell had pointed out one of the reasons why it was not sufficient, inasmuch as it did not provide for the propelling action of the driving-wheels on the rail. But, he might have added, it did not provide for the sliding action of the wheels. Upon inclines the wear and tear of rails was much greater than on a level road; and that was owing, not to any increase of weight, but to the different mode in which that weight passed along the surface of the rails. If the load merely rolled over the surface, the effect upon the rails was very different to what it was if the same weight slid over it, burnishing or trying to dislodge every particle from its

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neighbour. The machine therefore appeared to him to be incomplete, unless some means of testing that sliding action upon the rail were provided in addition to the rolling action. If stopping the rotation of the wheels would not lead to the expenditure of too much power, it would provide a means of subjecting rails in a short space of time to a test which, in the ordinary working of a railway, would only be obtained after some months, or even years, of trial. But if any railway company were public-minded enough to devote a length of their main line, where there was a steep gradient or a terminal station for experimental purposes, placing the same at the disposal of engineers to put down their rails, such a trial would be as valuable, perhaps, as any that could be desired.

Mr. R. Price Williams said, during a recent visit to Ireland the Author had kindly afforded him an opportunity of examining this rail-testing machine whilst at work, and he was glad to bear testimony to the admirable way in which it acted, and to the thoroughly reliable nature of the results arrived at by this new mode of testing rails. He had since been enabled to avail himself of this machine for testing some rails under somewhat peculiar circumstances. An eminent firm of iron merchants had undertaken a large foreign order for rails. These were duly forwarded to their destination; but after a short time complaints were made that the rails were of an inferior quality; and some of them that had become thoroughly worn out, with others which had not been used, were returned, with an intimation that they were not of the stipulated quality. The manufacturers of the rails, on the other hand, persisted in stating to the contrary. It was under these circumstances that he was called upon to give his opinion not only as to the quality of the rails, but to satisfy himself as to their identity with those supplied by the contractors. No more favourable opportunity than this could have presented itself for practically testing the merits of this machine. He accordingly had some of these returned but unused rails sent to Mr. Price; but before doing so, he had the ends of each rail cut off, polished up, and afterwards subjected to the acid test. The remaining rails, both worn out and unused, their ends being similarly cut off and treated with acid, were tested under his direction, at Mr. Kirkaldy's testing works. The results of the testing by Mr. Price's method proved conclusively the inferior quality of the material, each of the rails having been destroyed by the passage over them of 415,250 tons, at a speed of 15 miles an hour, giving as the measure of their life only 6,228,700 'speed-

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tons,' the life of an ordinary iron rail of fair average make being by this concentrated form of rolling test fully 30,000,000 'speed-tons.' Pieces of these rails tested by the machine showed the defective nature of the welding and the inferior quality of the material generally, which might be judged from the way in which the thin portion forming the top slab was crushed out and separated from the other portion of the head of the rail. Figs. 1 and 2, Plate 14, were representations of the sections of these rails after being subjected to the acid test. Figs. 3, 4, and 5 showed the structure of the returned worn-out rails. The great similarity in the make of these new and worn rails, as disclosed by the acid test and by actual wear, were apparent. The result of the experiments at Mr. Kirkaldy's, to test the quality of the unworn rails under a gradually increasing bending strain, afforded equally conclusive evidence as to the inferior quality of these rails, the ultimate stress borne by the unworn rail, Fig. 2, under a gradually increasing load applied at the centre of a 5-feet bearing being only 15 tons, whilst the elasticity of the rail was permanently injured with a strain of only 8 tons, as would be seen by the following Table:—

DEFLECTION under a gradual Bending Stress at the centre of a 5-feet Bearing.

——	Stress.	Deflection.	Permanent sets.	Remarks.
	lbs.	Inches.	Inches.	
Rail Section, Fig. 2, Plate 14.	10,000	0·12		
	11,000	0·13		
	12,000	0·14		
	13,000	0·15		
	14,000	0·17		
	15,000	0·20		
	16,000	0·23		
	17,000	0·25		
	18,000	0·28	0·03	Elastic strength 18,300 lbs. = 8·17 tons.
	19,000	0·36		
	20,000	0·42		
	21,000	0·50		
	22,000	0·63	0·32	
	23,000	0·76		
	24,000	0·98		
	25,000	1·35		
	26,000	1·76	1·38	
	27,000	2·24		
	28,000	2·90		
	29,000	3·83		
	30,000	5·03	4·50	
	31,000	6·14		
	32,000	8·35		
	32,588	9·72	..	Broke; ultimate strength 32,588 lbs. = 14·5 tons.

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The rail-testing machine of Mr. Price possessed one other advantage, and that was the rapid way in which the testing was accomplished; the entire time occupied in testing the rails he had referred to scarcely exceeded a fortnight. He considered this machine supplied an acknowledged want, and afforded the means of practically solving some important problems connected with permanent way. The first he should like to see solved would be the determination of the most suitable form for the head of the rail; for, so far as regarded the general form of the rail itself, the introduction of steel rails had in a somewhat unexpected manner practically determined it, and demonstrated that the double-headed form at least was a mistake, and unsuited for heavy main-line traffic; and that the Vignoles section supplied all the essential requirements of a good rail. Great as the wearing power of steel rails was, still, under the enormous amount of wear they were subjected to on some railways, their life if measured by time was very limited, and instances had occurred within his own knowledge where steel rails after seven or eight years' wear had already been renewed. The only worn-out rails that had as yet come under his notice were of the double-headed section.

Figure 6, Plate 14, showed the original and the reduced section of a double-headed steel rail laid down about seven years since in a tunnel on a railway subject to heavy traffic, and where high speeds were maintained. The top table had been quite worn away; but in addition to this the indentation in the lower table of the rail where it rested on the chair was so serious as to render these rails unfit to be turned; they had consequently been replaced with new steel rails, and the double-headed form had been definitely abandoned. On the Great Northern, Lancashire and Yorkshire, Midland, and Manchester, Sheffield and Lincolnshire railways a single-headed rail of the form shown in Fig. 7 had been recently substituted for the double-headed rail: it was a transition form which he thought would ultimately give place to the Vignoles section. The chief recommendation of the form of rail adopted on these railways was that a large amount of material, hitherto valuable only as affording vertical strength, was now made available as additional wearing surface. This concentration of the material, however, in the head of the rail had the effect of seriously weakening the rail as a girder, more especially at the joints, where it had been found necessary to provide additional strength, by the adoption of a stronger form of fish plate.

Another problem he hoped the rail testing machine would solve

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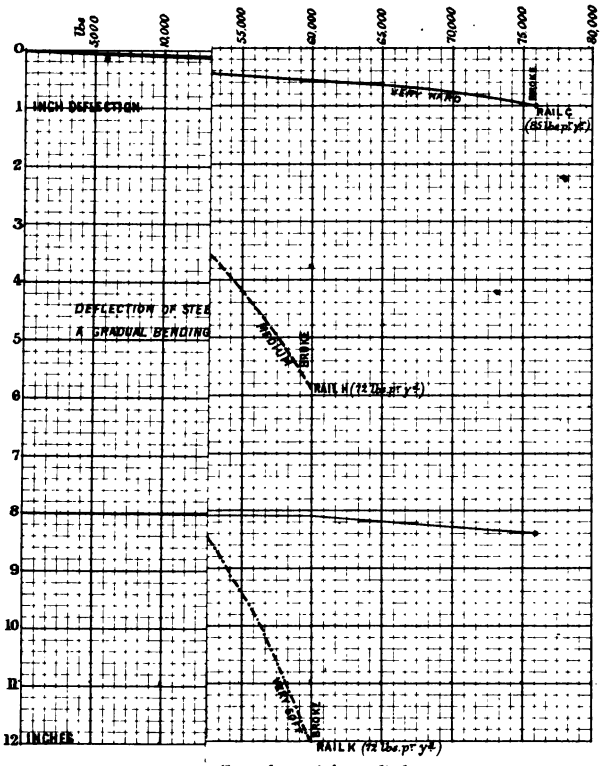
RAIL TESTS.



Shpails, with a weight applied

Fig. 6.

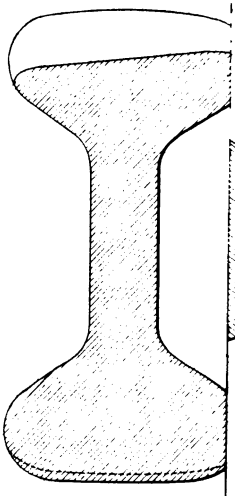
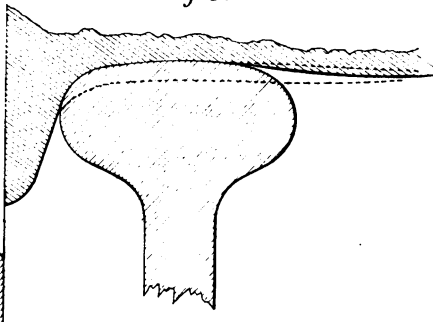


Fig. 8.



KELL, BRO.S LITH.rs CASTLE ST, HOLBORN.

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was, as to how far the durability of the rail was affected by the quality of the ballast. He felt satisfied that the result would show that it had more influence in this respect than was generally imagined. He had recently inquired into the life of ballast, and found that the wear of some kinds was very rapid indeed, more particularly the top ballast, owing to the constant attrition caused by the impact of the trains, and to the waste occasioned by the wind created by the rapid motion of the trains; he had known instances where the average life of the ballast was certainly not greater than that of the rails.

The form of the tire had an important relation to its own durability and to that of the road. He thought that the conical form of the tire was a mistake, and that much of the destructive effects upon both the road and the rolling stock was traceable to this conical form. As an instance of its prejudicial effect he might mention, that some years ago he had occasion entirely to renew a considerable length of straight and level road, with heavier rails, sleepers of large scantling placed nearer together, and ballast of the best description. When the new road was completed, and apparently in the highest condition for running, he was surprised to learn from the drivers of the express trains, that they were invariably obliged to slacken speed when running over that particular bit of road; and that immediately the train got on to the older portions of the road the running became quite steady again. This at once led him to suspect, what ultimately proved to be the fact, that the unsteady motion was chiefly due to the conical form of the tire, a slight amount of play in the bearings being sufficient on a true and level road to produce a swaying lateral motion which rapidly accumulated and produced the dangerous effects stated.

It would appear, therefore, that the inequalities in the road had in reality a tendency to prevent the accumulation of this side motion due to the play of the tires which was probably the reason of its not having been so much noticed heretofore. With the view of determining the best form for the head of the rail, he had, a few years ago, obtained from the locomotive superintendent of the Great Northern railway a number of profiles of worn tires, and had been able to obtain from a great number an average outline shown in Fig. 8, the rail-head being formed as closely as possible to fit it; the result was a much flatter rail-head than had hitherto been used on that line. He might mention as a singular coincidence, that Mr. F. W. Webb, Assoc. Inst. C.E., had informed him that in order to arrive at the correct form of the tire, he had

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collected a great number of worn rail templates, and from these obtained a mean outline, which, when compared with that derived from the series of worn tires, was found to correspond very closely.

He fully agreed with the Author as to the insufficiency, he might almost add absurdity, of the falling weight test; and in support of this view he would refer to the decided opinion expressed on a former occasion by Mr. Longridge, M. Inst. C.E.[1] There was one objection to the falling weight test which had not been referred to—the absence of any reliable method of accurately ascertaining the amount of deflection. In his opinion the next best method to that afforded by Mr. Price's testing machine was the dead weight test, which, if properly carried out, would enable any one to judge of the sufficiency of a rail to resist impact, the small amount of deflection and set, in a very hard rail, being alone sufficient to show that it was unfit for its purpose.

Fig. 9, Plate 14, showed the results of a gradual bending stress —a dead weight test—upon three double-headed rails, selected out of a great number of other rails, and tested for him at Mr. Kirkaldy's works. The first (C) had undergone a very small amount of deflection and set, and although it bore an exceedingly great strain, it was very hard and brittle, and manifestly unsuitable for a rail. The specimen marked (H) showed a considerably larger amount of deflection, and represented in his opinion all the essential qualifications required in a steel rail, viz., a sufficient amount of hardness to withstand the excessive wear, combined with the requisite amount of elasticity to prevent fracture by a sudden blow. The third specimen, marked (K), represented a class of rail which, although it might perhaps stand the severest test by the falling weight, was still wholly unsuited for a rail subject to heavy traffic, being evidently of too soft and tensile a quality of steel, with too small a percentage of carbon; it would be observed that the amount of deflection borne by this rail with a 5-feet bearing was fully 12 inches.

Mr. A. A. LANGLEY wished to bear his testimony to the practical efficiency of the method described by Mr. Price for determining the super-elevation of the outer rail on curves, and stated that he had devised the same method two years ago, and had since used it on various lines with great advantage. It also gave the proper elevation on the straight line before the curve was reached. It was a rule which should be more generally known;

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxvii., p. 390.

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but it was quite novel in practice. Plate-layers seldom understood the meaning of the term, "radius of a curve," but by this simple mode they could get the super-elevation at any given point of the curve, and on the straight line leading to the curve.

Mr. E. A. COWPER observed that many years ago he had constructed a machine for the late Mr. G. B. Thorneycroft, for the purpose of testing rails by actual wear caused by a rolling load. This machine was made, according to Mr. Thorneycroft's wish, of two rings or hoops of rails, about 10 or 12 feet in diameter, placed in chairs on sleepers, which were secured to two cast-iron wheels mounted on a horizontal shaft, so that when the shaft was driven by a powerful engine, the two rings formed of rails constituted in fact a line of railway that was itself running. Then, in order to imitate as closely as possible the precise action of the wheels of a train on the rails, a pair of ordinary running wheels, with their axle, axle-boxes, and springs complete, were fixed under a frame, exactly resembling half of a wagon, which was capable of being loaded. Next there was a hook fixed in a stationary beam, to which one end of the frame was attached, and it was so placed that the running wheels came exactly over the centre of the horizontal shaft, carrying the rails as before mentioned, so that the only difference in the action of the machine and an ordinary railway would at first sight appear to be that the rails ran under the wheels, in place of the wheels running over the rails; the whole action of the springs, the ordinary weight of wheels, axle-boxes, and springs being the same as on a railway; and what was perhaps as important a matter as anything else, the action of the flanges of the wheels was the same, as the wheels could roam about on the rails, as in practice, the flanges only keeping them in place. He, however, considered at the time that the fact of the rails having to be heated, and bent accurately to a circle, was practically a very objectionable feature in the machine, as unless the rails were properly curved, most unfair jumping would take place at high speeds, depending in degree upon the accuracy of the curvature in each case. Besides this the surface contact was less, owing to the curvature of the rails, and although this might have been allowed for to some extent, by increasing the size of running wheel, it would have complicated the consideration of the matter and the question of proper weight of wheel, and, again, the arched form of the rail no doubt gave it undue stiffness.

He then suggested a circular railway of the usual 4 feet 8½ inches gauge, formed of two ordinary rails, either with one wheel

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loose on the axle, or with one wheel smaller than the other, for the railway to run under. He did not, however, press the matter, as he felt the conditions would vary considerably from those of a railway; first, from the rails having been heated and bent and not being accurate to a circle; secondly, from the rails not having the same strains brought on them as when straight, and held down by distant chairs, which would have the effect of assisting to prevent deflection between the two chairs immediately before and behind the loaded wheels; thirdly, from the rails, when curved, having a certain amount of torsion and other strains brought on them, which they were not subjected to in practice, and which would cause much more deflection than in practice; and fourthly, from the action of the flanges of the wheels on the rails being different, partly from the wheels being either different in diameter, or from one being loose on the axle, in place of being rigidly fixed on it, and partly from the rails being most probably not nearly so accurate to a circle as straight rails were to a straight line.

He did not like the idea of substituting a solid, cast-iron cylindrical roller, like a heavy 'edge runner,' for the ordinary weight and spring of wrought-iron wheels and axle pressed down on the rail only, by springs and axle-boxes loaded with a heavy weight on the wooden platform of the wagon, as the yielding and springing of an ordinary wagon was very great, and altogether different from the hammer-like blows of a solid roller like an 'edge runner.'

In trying a rail by any wearing-out machine, he should be sorry also to give up the severe action of the flange of the ordinary wheel, which had been alluded to by the Author, but if rails were to be tried straight all flange action must be given up, and the weight would never come near the edges of the rails. Then there arose another question, viz., how was the heavy roller to be brought on each rail without an artificial blow or shock? The rail off which the roller came could not lie in the same part of the same chair as the rail on which it came, and there must of necessity be some inaccuracy, the chair and sleeper under it not being already in compression. If, however, it should be found in practice that the machine now proposed answered the purpose of demonstrating which were the rails that would stand best, and approximately how long they would stand with a given traffic, even although not strictly fulfilling the conditions of a railway, he thought it would be a most valuable machine, and one that would soon be used in testing the rails for large contracts, as makers of good nails would soon put up such a machine, if they

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could prove to the directors of a company that they made good rails, that would stand for a long term of years.

Mr. VIGNOLES, President, said it was now fully forty years since he designed the rail that bore his name, and had it manufactured so as to insure hardness of the top, and toughness at the bottom, to dispense with chairs, and to avoid making those holes or notches in the flanges of the rails which it had been properly said diminished the strength at least 25 per cent. Those were the merits he claimed for this rail when it was originally introduced. It was long adopted, in the first instance, as a contractor's rail; and he was glad to find after many years it was beginning to be better appreciated by English railway engineers. He had laid down this form of rail to a large extent in foreign countries, where it was now almost universally and exclusively adopted.

Mr. G. BERKLEY observed that he was not sanguine as to the good effects of this machine. The testing of rails might be considered as having either of two objects; first, to determine the practical length of life of certain rails in comparison with those of a standard quality; or, secondly, to ascertain whether a contractor was supplying the specified quality of material and workmanship.

With respect to the former of these objects, the system and machine described in the Paper appeared to him imperfect; as although the action of the machine represented the wear due to a rolling load, the effects of the adhesion of the driving wheels of the locomotive, of skidding, and of lateral and of horizontal blows, were not produced. He regarded the commonly adopted plan of subjecting rails to an extraordinary amount of duty, in certain exceptional positions near the entrance to stations, as much more satisfactory.

With respect to the latter object, it was difficult to settle tests which manufacturers could carry out, without involving a considerable addition to the cost of heavy and low priced materials. These tests must be such as could be cheaply and easily applied, and as would not seriously inconvenience the manufacturer, or delay the execution of a contract. He feared that the machine described was not suitable for checking the proceedings of a manufacturer during the execution of a contract. He agreed with the Author that the first consideration was to suit the rail to its work. The quality required would be influenced by various circumstances, and especially by the degree of cold to which the rails would be exposed. In hot climates a comparatively hard rail might be employed, and a greater amount of duty might probably be obtained than from a softer rail. He had sent

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rails to India which he thought better, as being less brittle, than others, but having made inquiries he found those which he had considered inferior proved the more durable.

Solidity was an absolute essential in a rail, and could be obtained in a degree in iron rails by a judicious arrangement of the pile out of which the rail was rolled, and careful workmanship, but it was only perfectly secured in steel rails which were rolled out of a solid bloom. Toughness and hardness should, he thought, be so proportioned as to make a rail which would be the most suitable to the circumstances under which it would be used. His practice was to test these qualities by the application of dead weight, and by a blow from a falling weight. These tests, though they were not designed to represent the wear the rail would have to undergo, were, in his opinion, the best which could be applied, as they were the most simple and inexpensive, and severely tested the qualities of material and workmanship.

The following Table showed the results of the tests he had applied during the execution of a large contract to thirty-six Bessemer steel rails of the double-headed section, weighing 68 lbs. to the yard:

Deflection with 20 tons.	Permanent set with 20 tons.	Deflection with 1 ton falling 30 feet.	Specific Gravity of Steel in Rail.	Proportion of Carbon in Rail.	Tensile Strength of Steel per Square Inch.
Length of Rail 4 feet. Bearings 3 Feet 6 inches apart.		Length of Rail 4 Feet. Bearings 3 Feet apart.			
	Inch.	Inches.			Tons cwt. qrs. lbs.
Max. .	·54	·37	7¾	7·868	·47[1]	Max. .	45 19
Min. .	·16[2]	·00	5½	7·850	·33[3]	Min. .	33 16
Mean .	·25	·09	6⅝	7·855	·38	—	. 40 10 2 22

No doubt the action of carriages was not so injurious as that of engines to the rails; but with regard to wagons, he had found by careful experiments that those having a short wheel base, and

[1] This rail deflected under 20 tons pressure, ·22 of an inch; acquiring a permanent set of ·02 inch and a deflection of 6⅝ inches with 1 ton falling 30 feet. Tensile strength, 41·19 tons per square inch.

[2] Proportion of carbon, ·38. Tensile strength, 42·8 tons per square inch.

[3] This rail deflected under 20 tons pressure, ·23 of an inch; acquiring a permanent set of ·10 of an inch, and a deflection of 7½ inches. Tensile strength, 37·18 tons per square inch.

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loaded with weights hanging over the ends, produced more severe effects upon the rails than resulted from the passage of a locomotive over them.

Mr. J. PRICE, in reply upon the discussion, said he did not propose to curve or cut the rails in testing them, but merely to fasten them down; he had, however, tried experiments with curved and straight rails, and he found the deflection was precisely the same under both conditions, and they broke with the same amount of cutting away of the bottom flange.

With respect to the matter of 'breaking,' if necessary for experimental purposes, it was easy to apply a break to one of the rollers, sufficient power being available to impart motion when the roller was sliding. Though the action of the machine might not be exactly the same as the action of ordinary rolling stock, or even of engines in the dragging on the rails, he nevertheless adhered to what he said, that if ten rails were laid down, one of which was a standard, all the rails that wore out before the standard were inferior to it; and that the number of 'speed-tons' which each rail bore was the fair measure of the relative value of that rail compared with others. With regard to the dragging force exerted on the rails by the driving wheels of engines, this could be imitated by applying friction to one of the rollers or all, not enough to make them slide, but equal to the retarding force of the weight of the train. It was manifest that in this case the rail exerted the same force to make the roller turn, and was dragged to the same extent as the rails were under the driving wheel of a loaded train, the rule of action and re-action coming into force.

As to the suggestion, that the railways in the neighbourhood of London afforded a good opportunity for testing rails, it was not likely that any directors would consent that their railways should be employed as testing machines for rails to be used all over the globe; or that the public, if they knew it, would be well pleased to contribute their share to the number of tons exerted in wearing out and breaking rails of every imaginable shape and quality.

Mr. VIGNOLES, President, observed with reference to the rule Mr. Price had laid down to ascertain the proper elevation of the outer rail, adopting the radius as a function, that in the early times of engineering, curves were laid out upon the principle of so many inches per chain being set off, in proportion to the radius, to give the curve. Instead of the mathematical process of setting off ordinates for the curve it was done in this simple way. There was a curious coincidence of figures, though not of formulæ, between that plan and the rule given by Mr. Price.

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Mr. J. PRICE said that by a singular coincidence the chord was a chain in length for the ordinary gauge, but with a change of gauge it would be of a different length.

In pursuance of the notice on the card of the Meetings, it was proposed, and resolved unanimously—

"That, in order to insure a fuller attendance of members than could be obtained on Easter Tuesday, the meeting be adjourned until Tuesday evening, the 18th of April."

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April 18, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

No. 1,240.—"On the Archimedean Screw-propeller, or Helix, of Maximum Work." By Sir FRANCIS CHARLES KNOWLES, Bart., M.A., F.R.S.

IN reading an account of the performance of the Griffiths Screw-Propeller, the Author was struck by the fact that the blades work in great part in the lateral streams of the water, and have no action at all in the dead water behind the stern-post, where power properly applied ought to be necessarily the most efficient. On the other hand, as is well known, in the common screw-propeller, at all points near the axis, the power is almost wholly employed in churning the water, which no doubt led Mr. Griffiths to construct his screw without any blade at those points, as little loss of power would arise from the omission.

This led to the consideration whether it would not be possible to devise such a form of the helix as should possess all the feathering properties of the Griffiths screw, and yet avoid the great loss of power near the axis, a loss common to that and to the ordinary helix. But failing to discover any guide in principle to such a desideratum, or any reason for preferring one form to another, the Author at length perceived that there must be some fixed form which is better than any other, and so by degrees he was led to propose the following problem, which, if capable of exact solution, must evidently give all that is required.

The problem is as follows:—

"What is the form of the surface of the blade in a screw-propeller, for which the 'useful effect' or 'work done' is the greatest possible?"

Of this the subjoined is a solution.

Let x, y, z be the co-ordinates of any point in the required surface, z being in the axis of rotation of the propeller; θ the angle which the surface, or its tangent plane, at any distance $r = \sqrt{(x^2 + y^2)}$ makes with the plane of xy; $\left(\frac{dz}{dx}\right) = p$,

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$\left(\frac{dz}{dy}\right) = q$ the partial differential coefficients of z with respect to x and y; v the velocity of rotation of a point at distance $= a$ from the axis z (say the semi-diameter of the screw), and therefore that of a point at distance r will be $\frac{vr}{a}$; d^2S the element of the surface at the distance r from the axis; and u the velocity of the ship due to the propeller, the medium being supposed for the present to be non-resisting.

Then the resolved parts of $\frac{vr}{a}$ and u along the normal to d^2S will be $\frac{vr}{a} \cdot \sin(S, xy)$ and $u \cdot \cos(S, xy)$, or, $\frac{vr}{a} \cdot \sin\theta$ and $u \cdot \cos\theta$, and the difference of these will be the effective impulse perpendicular to $d^2S = \left(\frac{vr}{a} \cdot \sin\theta - u\cos\theta\right)$. Therefore, in the usual notation, k being a constant known by observation, the force of propulsion on the element d^2S of the blade will be

$$k\left(\frac{vr}{a} \cdot \sin\theta - u\cos\theta\right)^2 \cdot d^2S.$$

Resolving this force in two directions, parallel and perpendicular to z, the former component will be

$$k \cdot \left(\frac{vr}{a}\sin\theta - u\cos\theta\right)^2 \cdot d^2S \cdot \cos\theta,$$

and the "useful effect" or "work done" in the direction of z (or, the ship's way) will be

$$ku\left(\frac{vr}{a} \cdot \sin\theta - u \cdot \cos\theta\right) \cdot d^2S \cdot \cos\theta.$$

Now $d^2S = \frac{dx \cdot dy}{\cos\theta}$, therefore the work done by the element of the blade is

$$ku\left(\frac{vr}{a} \cdot \sin\theta - u\cos\theta\right)^2 \cdot \cos\theta \cdot \frac{dx\,dy}{\cos\theta},$$

and the work for the whole blade may be obtained by integrating twice, within the proper limits, once with respect to x and once with respect to y, this expression, that is:—

(1.) Total work done $= \int_x \int_y ku\left(\frac{vr}{a} \cdot \sin\theta - u\cos\theta\right)^2 dx\,dy.$

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Now, by the known and usual formulæ,

$$\cos\theta = \frac{1}{\sqrt{(1+p^2+q^2)}},\ \sin\theta = \frac{\sqrt{(p^2+q^2)}}{\sqrt{(1+p^2+q^2)}},$$

$$r = \sqrt{(x^2+y^2)}.$$

and substituting these values in the expression (1) then,

$$\text{Total work done} = \int_x \int_y \frac{k\,u\left\{\frac{u}{a}(x^2+y^2)^{\frac{1}{2}}(p^2+q^2)^{\frac{1}{2}} - u\right\}^2 dx\,dy}{1+p^2+q^2},$$

and this, by the conditions of the question, is to be a maximum.

Therefore, if (omitting $k\,u$ the constant) there is put the quantity

$$\frac{\left\{\frac{v}{a}\cdot(x^2+y^2)^{\frac{1}{2}}\cdot(p^2+q^2)^{\frac{1}{2}} - u\right\}^2}{1+p^2+q^2} = \mathrm{V},$$

the result must be

$$\int_x \int_y \mathrm{V}\cdot dx\,dy = \text{a maximum},$$

or, in the notation of the calculus of variations,

$$\delta\cdot\int_x \int_y \mathrm{V}\,dx\,dy = 0,$$

which, by application of the known method of variations, leads to the partial differential equation of the required surface:

$$\frac{d\left(\frac{d\,\mathrm{V}}{dp}\right)}{dx} + \frac{d\left(\frac{d\,\mathrm{V}}{dq}\right)}{dy} = 0.$$

Performing, then, the operations indicated by these symbols, and reducing, a partial differential equation is arrived at between z, x, y, p, q, which is resolvable into two factors

$$px + qy = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

$$\frac{2\,v}{a}\cdot(x^2+y^2)^{\frac{1}{2}}(p^2+q^2)^{\frac{1}{2}} - u + u\cdot(p^2+q^2) = 0 \quad . \quad . \quad . \quad (b)$$

The first of these (a), or $x\left(\frac{dz}{dx}\right)+y\left(\frac{dz}{dy}\right) = 0$, being integrated, gives $z = \int\left(\frac{x}{y}\right)$, the well-known equation for the family of ordinary helices of all possible forms. These are evidently the surfaces of least work.

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The second equation (b) being solved with respect to (p^2+q^2), gives

$$(p^2+q^2)^{\frac{1}{2}} = -\frac{v}{au}\cdot(x^2+y^2)^{\frac{1}{2}} \pm \left\{1+\frac{v^2}{a^2u^2}(\cdot x^2+y^2)\right\}^{\frac{1}{2}},$$

or, taking the upper sign,

$$(p^2+q^2)^{\frac{1}{2}} = \tan\theta = \left\{1+\frac{v^2}{a^2u^2}(\cdot x^2+y^2)\right\}^{\frac{1}{2}} - \frac{va}{u}(x^2+y^2)^{\frac{1}{2}}.$$

At the axis $x = 0$, $y = 0$, $\tan\theta = 1$, therefore $\theta = 45°$.

At the edge of the blade $x^2+y^2 = a^2$ and $\tan\theta = \left(1+\frac{v^2}{u^2}\right)^{\frac{1}{2}} - \frac{v}{u}$.

Showing that "the angle should be regulated by the proportion which is to obtain between v and u, the speed of the screw and that of the ship (supposed to move *in vacuo*)." When x and y are infinite, $\tan\theta = 0$, $\theta = 0$, and the surface of the blade is perpendicular to the axis.

If there be made $\left\{1+\frac{v^2}{a^2u^2}(x^2+y^2)\right\}^{\frac{1}{2}} - \frac{v}{au}(x^2+y^2)^{\frac{1}{2}} = \mathrm{U}$,

then $p^2+q^2 = \mathrm{U}^2$, and the equation to be integrated is—

$$\left(\frac{dz}{dx}\right)^2+\left(\frac{dz}{dy}\right)^2 = \mathrm{U}.$$

Now $q = (\mathrm{U}^2-p^2)^{\frac{1}{2}}$ $\therefore \frac{dq}{dx} = \frac{\frac{d\cdot\mathrm{U}^2}{2\,dx} - p\cdot\left(\frac{dp}{dx}\right)}{(\mathrm{U}^2-p^2)^{\frac{1}{2}}}$

$$= \frac{\frac{d\cdot\mathrm{U}^2}{2\,dx}}{(\mathrm{U}^2-p^2)^{\frac{1}{2}}} - \frac{p\cdot\frac{dp}{dx}}{(\mathrm{U}^2-p^2)^{\frac{1}{2}}}, \text{ and } \frac{dq}{dz} = -\frac{p}{q}\left(\frac{dp}{dz}\right).$$

Substituting these values in the usual equation of condition—

$$\left(\frac{dp}{dy}\right)-\left(\frac{dq}{dx}\right)+q\left(\frac{dp}{dx}\right)-p\cdot\frac{dq}{dz} = 0,$$

there is obtained, after reduction,—

$$(\mathrm{U}^2-p^2)^{\frac{1}{2}}\left(\frac{dp}{dy}\right)+p\cdot\left(\frac{dp}{dx}\right)+\mathrm{U}\cdot{}^2\frac{dp}{dz} = \frac{d\cdot\mathrm{U}^2}{2\,dx},$$

which comes under the form of the linear equation of partial differentials between four variables—

$$\mathrm{P}\left(\frac{dp}{dx}\right)+\mathrm{Q}\left(\frac{dp}{dy}\right)+\mathrm{R}\cdot\left(\frac{dp}{dz}\right) = \mathrm{S}.$$

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The integration of this is by Lagrange's method reducible to that of the three equations:

$$\int (\mathrm{P}\,dp - \mathrm{S}\,dx) = \alpha, \quad \int (\mathrm{P}\,dz - \mathrm{R}\,dx) = \beta,$$
$$\int (\mathrm{P}\,dy - \mathrm{Q}\,dx) = \mathrm{V},$$

the final integral being $\mathrm{V} = \phi(\alpha, \beta)$.

Here $\mathrm{S} = \dfrac{d \cdot \mathrm{U}^2}{2\,dx}$, $\mathrm{P} = p \therefore 2\,\mathrm{S}\,dx - 2\,\mathrm{P}\,dp = 0$; or, $d \cdot \mathrm{U}^2 - d \cdot p^2 = 0$, and $\mathrm{U}^2 - p^2 = a^2 = q^2 \therefore q = a$ and $p = (\mathrm{U}^2 - a^2)^{\frac{1}{2}}$. Substituting these values in the equation of condition of a complete differential $dz = p\,dx + q\,dy$, the result is $dz - a\,dy = p\,dx = (\mathrm{U}^2 - a^2)^{\frac{1}{2}}\,dx$, or, $d(z - ay) = \sqrt{(\mathrm{U}^2 - a^2)}\,dx$, and $z - ay = \phi\{(\mathrm{U}^2 - a^2)_1\, a\}$.

This differentiated with respect to a gives $y = -\phi^1(\mathrm{U}^2 - a^2{}_1\, a)$ between which two equations a may be eliminated when the form of ϕ is assigned. Also $p\,dy - a\,dx = 0$,

or, $$dy - \frac{a\,dx}{\sqrt{(\mathrm{U}^2 - a^2)}} = 0, \text{ and } p\,dz - \mathrm{U}^2 \cdot dx = 0,$$

or, $$dz - \frac{\mathrm{U}^2 \cdot dx}{\sqrt{(\mathrm{U}^2 - a^2)}} = 0, \text{ which lead to the same result.}$$

Thus it appears that there does exist such a surface as has been supposed, and that it has been determined in all its generality. The Author will not now attempt to select any form of the function ϕ, but will only observe that it may possibly be made to include some favourable physical condition. This surface has a property in common with the ordinary helix. If it be intersected by a cylinder concentric with the shaft the spiral thread, or trace, on the surface of this cylinder, will make a constant angle with all lines on the cylinder drawn parallel to its axis; for the value of $\tan\theta$, and that of θ depend only on the radius $\sqrt{(x^2 + y^2)}$ and not upon z. The essential difference is, that a plane through the axis cuts this surface in a curve, instead of in a straight line. This it is which enables a moderate angle to be obtained close to the shaft, where the crank exerts the greatest power, and the blade, not churning the water, does not cause the vibration observable in the common helix, while its hold upon the dead water is a clear gain in power.

Putting r for $(x^2 + y^2)^{\frac{1}{2}}$ above, the tangent of the angle θ, which the surface of the blade makes with the plane of rotation at the distance r from the axis, or,—

$$\tan\theta = \sqrt{\left(1 + \frac{v^2 r^2}{a^2 u^2}\right)} - \frac{v r}{a u},$$

where v is the rotatory speed of the edge of the blade, and u the

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speed of the whole system (screw and ship) in the direction of the keel or of the axis. From this there is readily deduced $\tan 2\theta = \frac{au}{vr}$. Making $\theta = \alpha$ at the distance a, then similarly $\tan 2\alpha = \frac{au}{va}$, so that $\tan 2\theta : \tan 2\alpha :: a : r$, and the property of this helix is $\tan 2\theta = \frac{a \tan 2\alpha}{r}$. The property of the common helix being $\tan \theta = \frac{a \tan \alpha}{r}$, it will be seen that for the same distances the angle of the latter is double that of the former. For this reason the Author proposes to call this surface the "hemi-helix."

The principle of the superior power of this engine can now be rendered evident; for, if $r = 0$, then $\tan 2\theta = \frac{a \tan 2\alpha}{0} = \infty\, ty$, $\therefore 2\theta = 90°$ and $\theta = 45°$. Thus, instead of a surface which near the shaft is almost parallel to the axis, there is a surface inclined at a medium angle, just where the crank exerts the greatest power. Again, $\tan 2\alpha = \frac{u}{v}$ and $u = 0 \tan 2\alpha$, so that to obtain the same speed u, the speed of rotation v may be reduced, and a longer stroke and a more powerful crank employed.

The Author has called the velocity u the speed "of the system;" for at present there is no special application of the machine to the propulsion of a ship more than to any other work which it may be capable of doing; so that in the calculation of its total force it is supposed to act *in vacuo*. This distinction must be carefully borne in mind, for when the machine is applied to a ship it is impossible that $\tan 2\alpha = \frac{u}{v}$, any more than in the common helix $\tan \alpha = \frac{u}{v}$, inasmuch as that u, the terminal velocity of the ship, can never be equal to u, the speed due to the same power *in vacuo*. There will be what is called "a slip" measured by $v \tan 2\alpha - u$, where u is now the actual velocity of the ship through the water, when greatest. It will be seen presently what this supposed "slip" really means.

Retaining the same notation, the total pressure on the surface of the blade in direction of the axis of rotation will now be computed, or, of the ship's way.

The element of the pressure was shown to be generally

$$\frac{k u^2 \left\{ \frac{v}{au} (x^2 + y^2)^{\frac{1}{2}} (p^2 + q^2)^{\frac{1}{2}} - 1 \right\}^2}{1 + p^2 + q^2} \cdot dx\, dy \quad . \quad . \quad . \quad . \quad (1)$$

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also that—

$$(p^2+q^2)^{\frac{1}{2}} = \left\{1+\frac{v^2}{a^2u^2}(x^2+y^2)\right\}^{\frac{1}{2}} - \frac{v}{au}\cdot(x^2+y^2)^{\frac{1}{2}} \quad . \quad . \quad (2)$$

Substituting this value in the expression (1) for the force, and reducing, then

Pressure in direction of the axis,—

$$= \frac{ku^2}{2}\int_x\int_y\left\{\left(1+\frac{v^2}{a^2u^2}(x^2+y^2)\right)-\frac{v}{au}\sqrt{\left\{(x^2+y^2)+\frac{v^2}{a^2u^2}(x^2+y^2)^2\right\}}\right.$$

The last term within the brackets is not integrable in finite terms; but, $\frac{v^2}{a^2u^2}(x^2+y^2)^2$ may be neglected in comparison with (x^2+y^2) under the radical without sensible error as $\frac{v^2}{u^2} = \frac{1}{\tan\cdot^2 2a}$ is very small for points near the axis, tan 2 a being almost infinite, and at more distant points it is considerable. This done there remains to be found,

$$\frac{ku^2}{2}\int_x\int_y\left\{1+\frac{v^2}{a^2u^2}(x^2+y^2)-\frac{v}{au}(x^2+y^2)^{\frac{1}{2}}\right\}dx\,dy.$$

Omitting $\frac{ku^2}{2}$ and integrating first with respect to x,—

$$x+\frac{v^2}{a^2u^2}\left(\frac{x^3}{3}+xy^2\right)-\frac{vx}{2au}\sqrt{(x^2+y^2)}-\frac{v}{2au}y^2\cdot\text{hyp. log}$$
$$\text{of}\,(x+\sqrt{(x^2+y^2)})+\phi y.$$

When $x=0$ the force $=0$ and $\phi y = \frac{vy^2}{2au}$ hyp. log y which gives the complete integral in x,—

$$x+\frac{v^2}{a^2u^2}\left(\frac{x^3}{3}+xy^2\right)-\frac{vx\sqrt{(x^2+y^2)}}{2au}-\frac{vy^2}{2au}\text{hyp. log.}$$
$$\text{of}\left(\frac{x+\sqrt{(x^2+y^2)}}{y}\right).$$

Similarly, integrating this with respect to y and determining ϕx so that the force may vanish with y, there is obtained for the complete integral, restoring the constant $\frac{ku^2}{2}$,

Force of pressure in direction of the axis,—

$$= \frac{ku^2}{2}\left\{xy+\frac{v^2}{3u^2u^2}xy(x^2+y^2)-\frac{vxy}{3au}\sqrt{(x^2+y^2)}-\frac{vx^3}{6au}\times\right.$$
$$\text{hyp. log}\left(\frac{y+\sqrt{(x^2+y^2)}}{x}\right)-\frac{vy^3}{6au}\cdot\text{hyp. log}\left.\left(\frac{x+\sqrt{(x^2+y^2)}}{y}\right)\right\}.$$

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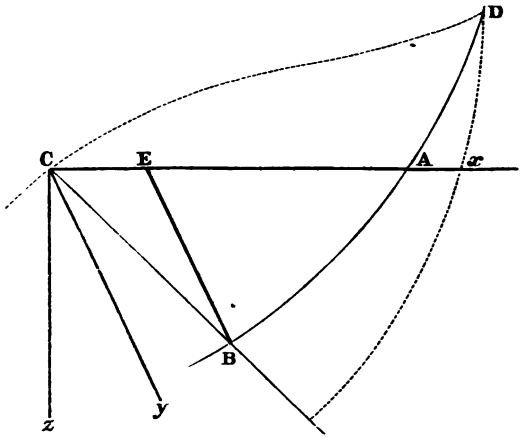
The edge of the blade being D B, cutting the plane of rotation in B, C z being the axis, and B E parallel to C y the co-ordinate axis of y; make A C B = β, which may be called the "amplitude"

Fig. 1.



of the blade, and the integral must be taken from C to A for x, and from A C parallel to C y, or, y for y, that is from $x = 0, y = 0$ to $x = a, y = a \sin \beta$ = E B. This gives finally:

Pressure in direction of the axis of the shaft

$$= \frac{k u^2}{2} \Big\{ a^2 \sin \beta + v^2 a^2 \sin \beta \cdot (1 + \sin^2 \beta) - \frac{v a^2}{3 u} \cdot \sin \beta \sqrt{(1 + \sin^2 \beta)} - \frac{v a^2}{6 u} \text{hyp. log } (\sin \beta + \sqrt{(1 + \sin^2 \beta)} - \frac{v a^2}{6 u} \sin^3 \beta \cdot \text{hyp. log} \left(\frac{1 + \sqrt{(1 + \sin^2 \beta)}}{\sin \beta} \right) \Big\}.$$

This may be put under the form $k(A u^2 - B u v + C v^2)$ by making $\frac{a^2 \sin \beta}{2}$ = A, $\frac{a^2}{6} \sin \beta \sqrt{(1 + \sin^2 \beta)} + a^2 \sin^3 \beta$ hyp. log $\left(\frac{1 + \sqrt{1 + \sin^2 \beta}}{\sin \beta} \right) + \frac{a^2 \sin^2 \beta}{12}$ hyp. log $(\sin \beta + \sqrt{1 + \sin^2 \beta})$ = B,

and $\frac{a^2}{6} \sin \beta (1 + \sin^2 \beta) = C.$

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If, as in the case of the common helix, H be the area of the effective midship section, $k\,\mathrm{H}\,u^2$ is the resistance due to velocity u of the ship; let W be the weight of the ship and cargo, t the time, and $g = 32\frac{1}{6}$ feet = the force of gravity; then, H being necessarily greater than $\frac{a^2 \sin\beta}{2} = \mathrm{A}$, for the variable motion of the ship under a constant force,

$$\frac{\mathrm{W}}{g}\left(\frac{du}{dt}\right) = k(\mathrm{A}u^2 - \mathrm{B}uv + \mathrm{C}u^2) - k\,\mathrm{H}\,u^2$$

$$= k\{(\mathrm{A}-\mathrm{H})u^2 - \mathrm{B}uv + \mathrm{C}v^2\}$$

$$= k\left\{\mathrm{C}v^2 - (\mathrm{H}-\mathrm{A})\left(u^2 + \frac{\mathrm{B}uv}{\mathrm{H}-\mathrm{A}} + \frac{\mathrm{B}^2v^2}{4(\mathrm{H}-\mathrm{A})^2}\right) - \frac{\mathrm{B}^2v^2}{4(\mathrm{H}-\mathrm{A})^2}\right\}$$

$$= k\left\{\left(\mathrm{C} + \frac{\mathrm{B}^2}{4(\mathrm{H}-\mathrm{A})}\right)v^2 - (\mathrm{H}-\mathrm{A})\left(u + \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}\right)^2\right\},$$

which gives

$$\frac{k(\mathrm{H}-\mathrm{A})g\,dt}{\mathrm{W}} = \frac{du}{\left(\frac{\mathrm{C}}{\mathrm{H}-\mathrm{A}} + \frac{\mathrm{B}^2}{4(\mathrm{H}-\mathrm{A})^2}\right)v^2 - \left(u + \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}\right)}.$$

Put $\left(\frac{\mathrm{C}}{\mathrm{H}-\mathrm{A}} + \frac{\mathrm{B}^2}{4(\mathrm{H}-\mathrm{A})^2}\right)v^2 = m^2$, then integrating,

$$\frac{2k(\mathrm{H}-\mathrm{A})gt}{\mathrm{W}} = \frac{1}{v\sqrt{\left(\frac{\mathrm{C}}{\mathrm{H}-\mathrm{A}} + \frac{\mathrm{B}^2}{4(\mathrm{H}-\mathrm{A})^2}\right)}}\,\mathrm{h.log}\left\{\frac{m + \left(u + \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}\right)}{m - \left(u + \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}\right)}\right.$$

$$\left. + \text{constant.}\right.$$

When $t = 0$, $u = 0$ and

$$\text{Constant} = -\frac{1}{m}\,\text{hyp. log}\left\{\frac{m + \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}}{m - \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}}\right\}.$$

When t is infinitely great the terminal velocity of the ship in the resisting medium is found by putting = 0 the denominator of the logarithm: this gives

$$u = v \cdot \sqrt{\left(\frac{\mathrm{C}}{\mathrm{H}-\mathrm{A}} + \frac{\mathrm{B}^2}{4(\mathrm{H}-\mathrm{A})^2}\right)} - \frac{\mathrm{B}v}{2(\mathrm{H}-\mathrm{A})}.$$

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Make H = 0 then the velocity in a non-resisting medium u'

$$= v\left(\frac{\sqrt{(B^2 - 4AC)} + B}{2A}\right)$$

and $\frac{u' - u}{u'}$ is the "slip." But $v \cdot \tan 2a = u'$ ∴

$$\tan 2a = \frac{B \pm \sqrt{(B^2 - 4AC)}}{2A}$$

which gives a in terms of a the radius and β the "amplitude."

The Author has said above, that what is called "the slip" is merely the difference between the speed of the ship through the water and the speed which she would have in a non-resisting medium under the same power.

The term is an unfortunate one, and calculated to confuse and to mislead. It induces a belief in some absolute imperfection in the screw itself, which causes a loss of power; indeed, one eminent writer (Dr. Main) expressly attributes it to "the yielding nature of the medium," as if the law of the equality of action and reaction did not apply to fluid media![1] The medium is unfortunately too unyielding. It does worse; it diverts attention from the real point of importance, as will be seen by examining the formulæ (A) attentively, namely, that relation between the form, position, and area of the blades, and the effective midship section of resistance in the ship, which should give the greatest speed with a given power.

In the common screw propeller the expression for the pressure is $k\beta(v \cdot \tan a - u)^2 \cdot A$, where $A = \cdot \frac{a^2}{2} \cdot (1 + \tan^2 a \cdot$ hyp. log

[1] "Slip of the Screw.—The screw-propeller, if working in a medium that could not yield, would advance a distance equal to the pitch during each revolution. But such is not the case in the yielding medium in which it actually does rotate, and, therefore, it is found that the knots per hour passed over by the ship are less than those which ought to have been passed over on the supposition that the screw was working in an unyielding medium. If, then, the former be taken from the latter, the difference is called the *slip of the screw in knots*; and if this difference be divided by the speed of the screw in knots, we shall have the fraction of the speed lost: this again, if multiplied by 100, will give the percentage of loss."[1]

This clearly implies that the reaction of the water on the surface of the blade is less than would be that of a solid nut, the rotatory forces in either case being the same. It is hardly necessary to refute this assumption, which ignores the universal equality of action and reaction.—F. C. K.

[1] "The Marine Steam-Engine," by Professor Main, and T. Brown, R.N., 4th edition (1860), p. 139, Art. 171.

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$\sin^2 a$), and the relation between v the speed of rotation of the blade at distance a, and u the speed of the ship, is obtained by assuming the pressure in direction of the keel to be equal to the resistance on the effective midship section H, or $= k\,\mathrm{H}\,u^2$, which thus gives $u\sqrt{\mathrm{H}} = \sqrt{\beta\,\mathrm{A}} \cdot (v \tan a - u)$. Then $v \cdot \tan a$ being the velocity of the screw in advance, as if it were working in a nut *in vacuo* without friction ($= u'$ suppose), u is found to be equal to the quantity $\dfrac{u' \cdot \sqrt{\beta\,\mathrm{A}}}{\sqrt{\mathrm{H}} + \sqrt{\beta\,\mathrm{A}}}$ and $\dfrac{u' - u}{u'}$, which is "the slip," is

$$= \frac{\sqrt{\mathrm{H}}}{\sqrt{\mathrm{H}} + \sqrt{\beta\,\mathrm{A}}} \quad \ldots\ldots\ldots \quad \text{(A)}.$$

Now, first, let it be observed that when the resistance becomes equal exactly to the power, the velocity becomes uniform. But, what is this but the "terminal" velocity, the limit of the variable velocity, which it continually approaches but never attains? And what is the above advance of the screw in a nut, if the ship be supposed to be pressed forward by it on a perfectly smooth surface, but the speed of the ship in a non-resisting medium?

This will now be proved by a direct determination of the motion as a variable motion produced in a resisting medium by a constant force. Retaining the former rotation,

$$\frac{\mathrm{W}}{g} \cdot \left(\frac{du}{dt}\right) = k\left\{\beta\,\mathrm{A} \cdot (u' - u)^2 - \mathrm{H}\,u^2\right\}.$$

This leads easily to

$$\frac{(\mathrm{H} - \beta\,\mathrm{A})\,k\,g\,dt}{\mathrm{W}} = \frac{du}{\dfrac{\mathrm{H}\,\beta\,\mathrm{A}\,u'^2}{(\mathrm{H} - \beta\,\mathrm{A})^2} - \left(u + \dfrac{\mathrm{B}\,\mathrm{A}\,u'}{\mathrm{H} - \beta\,\mathrm{A}}\right)^2},$$

which integrated gives

$$\frac{2\,k\,g\,t}{\mathrm{W}} = \frac{1}{u'\sqrt{\mathrm{H}\,\beta\,\mathrm{A}}} \times \text{hyp. log} \left(\frac{\dfrac{u'\sqrt{\mathrm{H}\,\beta\,\mathrm{A}}}{\mathrm{H} - \beta\,\mathrm{A}} + \left(u + \dfrac{u'\,\beta\,\mathrm{A}}{\mathrm{H} - \beta\,\mathrm{A}}\right)}{\dfrac{u'\sqrt{\mathrm{H}\,\beta\,\mathrm{A}}}{\mathrm{H} - \beta\,\mathrm{A}} - \left(u + \dfrac{u'\,\beta\,\mathrm{A}}{\mathrm{H} - \beta\,\mathrm{A}}\right)}\right)$$

$$+ \text{constant}.$$

As t and u begin together the constant is found,

$$(\mathrm{C\ say}) = \frac{1}{u'\sqrt{\mathrm{H}\,\beta\,\mathrm{A}}} \cdot \text{hyp. log} \left(\frac{\sqrt{\mathrm{H}} - \sqrt{\beta\,\mathrm{A}}}{\sqrt{\mathrm{H}} + \sqrt{\beta\,\mathrm{A}}}\right).$$

In order now to obtain the "terminal," or, limiting velocity,

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make t infinite, or, put the denominator in the logarithm = 0, which gives

$$u = \frac{u' \cdot \sqrt{\beta A}}{\sqrt{H} + \sqrt{\beta A}}, \text{ and } \frac{u' - u}{u'} = \frac{\sqrt{H}}{\sqrt{H} + \sqrt{\beta A}}$$

the very expression for "the slip." Now, in order to see what u' is, let H = 0, which gives a non-resisting medium, when $u = \frac{u' \sqrt{A}}{\sqrt{\beta A}} = u''$, $u' - u$ is therefore evidently the difference between the velocity of the ship in a non-resisting medium, and the actual terminal velocity in a resisting medium, as mentioned above; and $\frac{u' - u}{u'}$ is the ratio of the former to the latter, the measure of "the slip."

Mention has occasionally been made of "negative" slip in the reports of trials of the working of screw-propellers in men-of-war; but, as the expression for the slip is always positive, and as the terminal velocity can never be equal to the velocity in a non-resisting medium, still less exceed it, some explanation must be sought for the phenomenon. It can only arise from the joint action of steam and sails. In this case it is possible; for, if a new power P is introduced, this quantity must enter into the denominator of the logarithm, and then the terminal velocity due to both powers acting together may well be greater than the velocity *in vacuo* due to one of them alone. The modification of the formulæ to meet this case is easy, and their interpretation afterwards ready and conclusive.

In conclusion, repeating what has been said above, the chief point is to have the terminal velocity the greatest possible. Now, its value above found is $u = \frac{u' \cdot \sqrt{\beta A}}{\sqrt{H} + \sqrt{\beta A}}$; and $a\,\beta \tan a = l$ where l is the length of the screw, $\therefore \beta = \frac{l}{a \tan a}$ and $\beta A = \frac{a l}{2 \tan a} \cdot (1 + \tan^2 \cdot a \cdot \text{hyp. log} \cdot \sin^2 a)$; H is constant; so, if l be assumed to be given, it is necessary to make u a *maximum* with regard to a. Since $v \tan a = u'$ $\therefore$ there remains to find what value of a will make $\frac{v \cdot \tan a \sqrt{\beta A}}{\sqrt{H} + \sqrt{\beta A}}$ the greatest possible.

This can be done with near approximation, and if a be the angle obtained, a screw with that angle and length l will give, with a

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given power, the greatest speed attainable by the ship to which it is applied. In this sense only does what has been improperly called "slip" become a measure of practical importance. The Author proposes to call $\frac{u}{u'}$ "the ratio of resistance," and to drop the term "slip" as fallacious.

The simplest property of the screw-propeller of greatest work (which the Author has called the "Hemi-Helix") is

$$\tan 2\theta = \frac{a \cdot \tan 2\alpha}{r}.$$

From this equation the surface may be constructed, as the common helix is from its equation $\tan \theta = \frac{a \cdot \tan \alpha}{r}$.

[Mr. W. Froude

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Mr. W. Froude remarked, through the Secretary, that he felt great hesitation in offering what would be regarded as an adverse criticism of an elaborate theoretical discussion embodied in technical mathematics, presented by a writer whose command of deductive mathematical machinery was, he doubted not, superior to his own. Nevertheless the subject was one to which he had long devoted much thought and research, and in the investigation of which he had enjoyed opportunities of testing his views both by varied observation and direct experiment, so that the remarks he had to make were by no means the expression of hasty or immature conclusions. On the other hand, it was to the fundamental principles from which the discussion proceeded, and not to the discussion itself, that his remarks would apply. He must also frankly add, that those principles, which he should point to as erroneous, used to be almost universally promulgated in mathematical treatises of reputed authority; and were very commonly even now relied on as the basis of mathematical reasoning, by those whose independent investigations and experimental researches had not obliged them to detect their unsatisfactory character.

He referred, in the first place, to what might be called the very foundation of the Author's deductive process,—the proposition, that when a plane moved obliquely through a fluid at a given velocity, the normal pressure on its surface was as the square of the sine of the angle of obliquity.

He referred, in the second place, to the hypothesis that when the true law of pressure on a plane thus moving was in any way determined, it was legitimate to determine the local pressure on each unit of area in a curved surface moving through a fluid, by applying that law to the unit in virtue of the angle presented by its tangent plane to the plane of rotation.

Thirdly, he referred to what appeared to be a misconception of the dynamical relations, or inherent conditions of the slip of the propeller.

First, as regarded the proposition that the normal pressure on an obliquely moving plane was as the square of the sine of the angle of obliquity. The basis on which the assertion of this proposition usually rested would be best explained by reference to the accompanying diagram (Fig. 2):

Let the line $a\,b$ represent the sectional or edgeways view of a given plane immersed in a fluid, and let the plane be moving through the fluid, and traversing successively the positions $a\,b$ and $a'\,b'$, so that the space $a\,a'$, or $b\,b'$ was traversed in an unit of time; then

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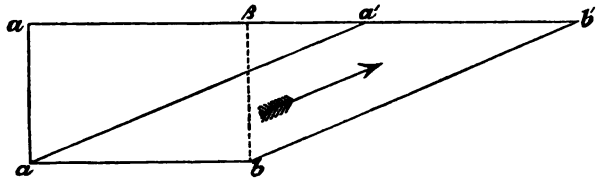
$a\ a'$, or $b\ b'$ would be the velocity of the plane in the line of motion through the fluid, which might be called (v).

Fig. 2.



It was common to regard this velocity as resolved into two components: one edgeways in the direction of $a\ b$, the other normal in the direction of $a\ \alpha$, and in the diagram $a\ \alpha$ was the measure of the component.

It was then held that the edgeways component was inoperative as a condition of resistance, in the same sense in which it might be fairly said that if the motion were purely edgeways the motion would be unresisted (for in this phase of the discussion, surface friction might be not improperly disregarded), while as regarded the normal component of the velocity, it was held that this alone was operative as creating resistance; and since the distance $a\ \alpha$, or $b\ \beta$, expressed the normal component of the velocity, and since it was held that when the motion of a plane was purely normal the resistance experienced was as the square of its velocity, it was inferred that the pressure on the plane would be as ($a\ \alpha^2$). Now if θ was the angle between the plane and the actual line of motion, ($a\ \alpha$), or ($\beta\ \beta'$) = v sin. θ, and the result was equivalent to the expression, Resistance = $k \times$ area $\times\ v^2$ sin.$^2\theta$; and it must be admitted that there was, at first sight, much plausibility in this mode of viewing the question.

Yet a very little reflection on the proposition in the aspect in which the diagram presented it, showed at once that the quasi reasoning by which it was arrived at was open to grave question, since it was obvious that the fluid to the right hand of the plane, into which the plane was continually intruding itself edgeways, was in a very different condition from that immediately in front of it. For it was certain, both as a matter of observation and of inference, that when the motion of the plane was purely normal, the mass or volume of fluid immediately in front of and behind it, must be, and was, moving forward with it, and almost with exactly the same velocity; whereas the fluid on either side of it was comparatively undisturbed, so that when the edge of the plane intruded

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itself into this undisturbed region, it must experience an excess of pressure, due to the normal motion which it at once imposed on the fresh particles which it pushed forward in front of it, and dragged forward behind it.

This operation indeed was a matter of common experience, and was unhesitatingly remarked on by those who witnessed it, as a natural and obvious consequence, when the circumstances presented themselves under a suitable aspect, and when their inherent contravention of the accepted law of the square of the sine was not perceived.

Thus, when a vessel was working to windward, immediately after she had tacked and before she had gathered headway, it was plainly visible, and it was known to every sailor, that her leeway was much more rapid than after she had begun to gather headway. The more rapid her headway became the slower became the lee drift; not merely relatively slower, but absolutely slower.

Again, any one might obtain conclusive proof of the existence of this increase of pressure, occasioned by the introduction of the edgeways component of motion, who would try the following simple experiment. Let him stand in a boat moving through the water, and, taking an oar in his hand, let him dip the blade vertically into the water alongside the boat, presenting its face normally to the line of the boat's motion, holding the plane steadily in that position, and let him estimate the pressure of the water on the blade by the muscular effort required to overcome it. When he has consciously appreciated this, let him begin to sway the blade edgeways like a pendulum, and he will at once experience a very sensible increase of pressure. And if the edgeways' sweep thus assigned to the blade is considerable and is performed rapidly, the greatness of the increase in the pressure will be astonishing, until its true meaning has been realised. Utilizing this proposition, many boatmen, when rowing a heavy boat with narrow-bladed oars, were in the habit of alternately raising and lowering the hand with a reciprocating motion, so as to give an oscillatory dip to the blade during each stroke, and thus obtained an equally vigorous reaction from the water, with a greatly reduced slip or sternward motion of the blade.

The attempt to quantify this increase of pressure, or in other words to assign a true law of resistance to an obliquely moving plane in virtue of the obliquity, was, he believed, an extremely difficult problem, and one which had not yet been regularly solved. He had himself attempted and obtained an approximate solution, the steps of which it would take too long to discuss here; and it

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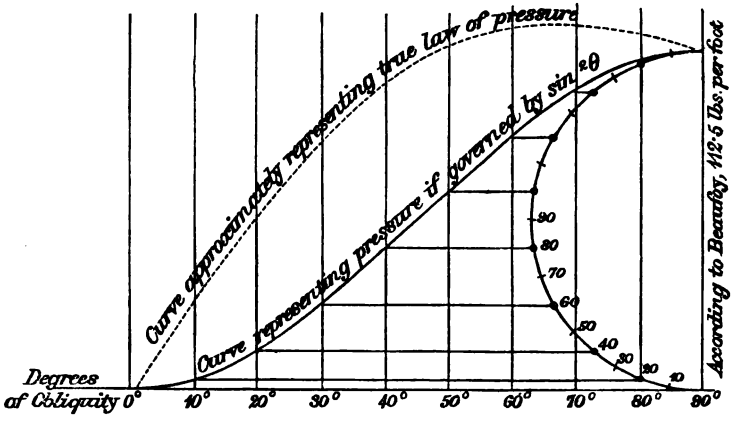
was the less necessary to do this, as he hoped some of the advanced mathematicians of the day would shortly direct their attention to the question.

He believed, also, that no really satisfactory series of experiments had ever been tried by which an empirical law could be laid down. Hutton's experiments on the resistances of surfaces of various shapes moving obliquely through air gave some suggestions towards the formation of such a law; and something might be gleaned from some experiments of Vince's, performed with small discs moving obliquely through water, though the discs were too small to give decisive results, and the mode in which the pressures were determined was far from satisfactory.

Beaufoy, indeed, tried a series of experiments in water with planes of sufficient area; but his method of experimentalising was by no means satisfactory, and the results, when analysed, presented extraordinary anomalies (explicable, perhaps, by the faults of the method), which rendered a large part of them certainly unacceptable.

Relying, however, on such results as could be gleaned from the several sets of experiments referred to, and on the support which these furnished to the approximate solution which he had himself deduced, he ventured to indicate what he believed to be the character of the true law, in comparison with that depending on the squares of the sines of the obliquity.

Fig. 3.



He could best do this by the annexed diagram, Fig. 3, in which the spaces on the abscissa represented the various angles of obliquity,

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with which the plane—say a square plane of 1 foot area—might be supposed to be moving, and the ordinates represented the corresponding normal pressures in lbs. experienced by the plane; the motion being supposed to take place in water, and the velocity of motion to be in every case the same, namely, 10 feet per second.

The continuous line represented, thus, the accredited law of resistance as regulated by the squares of the sines, and the dotted line represented in character the true value of the resistance.

The curve representing (k sin.$^2\theta$) was readily drawn, if it was borne in mind that sin.$^2\theta = \frac{\text{vers } 2\theta}{2}$.

It must be observed, in reference to the dotted line, which represented the true law of resistance, that it must vary somewhat, in terms of the variation, in the form of the plane and the character of the movement of the plane in relation to that form. Thus, the curve would not be the same for an oblong rectangular plane as for a square plane of the same area. And with an oblong plane, the curve would be different according as the plane was supposed to be moving obliquely edgeways or obliquely lengthways.

The figure 112·5 lbs. per square foot, as the resistance of a plane when its motion was purely normal through water and its speed 10 feet per second, was quoted from Beaufoy's results.

As regarded the general aspect of the comparison between the suggested approximately true law of pressure, and the erroneous law which represented it as governed by (sin.$^2\theta$) several instructive features might be confidently accepted.

(1.) Indubitably, according to the true law, the pressure was far more nearly proportionate to (sin. θ) than to (sin.$^2\theta$); but there was good reason to think that the magnitude of the pressure at oblique angles was understated, even when it was held to be proportional to the simple power instead of to the square of the sine. For angles of very high obliquity the latter law understated it almost infinitely, and this difference was indicated by the diagram.

(2.) According to the suggested law, an apparently paradoxical result arose with angles of small obliquity; the result, namely, that by slightly inclining a plane to the line of motion the normal pressure which it experienced was slightly increased instead of being diminished; and even its resistance in the line of motion was thus increased if the obliquity was very small. The theoretical aspect of the conditions inherent in the intrusion of the plane into undisturbed fluid, in virtue of the superadded edgeways component of motion, pointed to this quasi-paradoxical conclusion;

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and Hutton's experiments distinctly, and Vince's and Beaufoy's, though less clearly, presented indications of its truth.

(3.) The excess of resistance experienced by a plane moving with a high degree of obliquity explained the otherwise inexplicable phenomenon of the support offered by the air as an inclined plane to the rotating Australian weapon 'boomerang,' which, in virtue of its rapid rotation, virtually or approximately realised a normal resistance to normal motion, as great as that due to the whole area covered by it while rotating, even when rotating locally and without progression edgeways through the air; and probably a far greater normal resistance when the edgeways motion was superadded. Whereas, according to the popular law which referred the normal pressure to $(\sin.^2 \theta)$, the normal motion should be as rapid when the weapon was spinning and travelling edgeways as when at rest.

It was in virtue of this same condition of rotation, combined with progression, that a screw propeller, consisting of a pair of comparatively narrow sectorial blades, realised the reaction (available for propulsive action) due to the whole 'disc of rotation;' or perhaps it would be more correct to say, due to the dynamic resistance of the whole cylindrical column of fluid traversed by it in its rotative progression. In point of fact, a two-bladed screw of ordinary proportions rotating locally without progression, at a speed due to a given velocity of progression, experienced about as much propulsive pressure as if it consisted of blades wide enough to occupy the complete circle, and, instead of rotating, were being pushed sternward with that given velocity; and when, by the ship's advance, it was constantly being carried forward into undisturbed fluid, its efficiency of resistance was still farther increased.

The second point to which he objected in the Paper was the hypothesis that when the true law of pressure on a plane moving obliquely through the water had in any way been determined, this law could be applied separately to each separate unit of a curved surface moving through the water, taking, as the basis of the application, the angle between the line of motion and the tangent plane of the unit. The mathematical discussion of the pressure on each point in the surface ought to take account, not merely of the tangential direction of the surface at that point, but also of its position in the surface as a whole, and of its relation to all other points in the surface; for the operation of each point in the surface on the whole volume of fluid through which the surface was moving modified the pressure which the fluid exerted on all other points. The discussion, to be real and complete, must

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take account of the surface as a whole, and of the universe of fluid which surrounded it; and though, no doubt, the volumes of fluid nearest to the surface were those which were most operative in governing the pressure, it was quite impossible to proceed in the investigation by the simple differential method which the Author had adopted.

He had, himself, long since seen this to be the case, but to effect the solutions he desired, by regular mathematical methods, applied to the principles which he saw and appreciated, was beyond his powers; and it was only recently that he had become acquainted with some of the results which the higher mathematicians of the period had achieved in this direction; and he was neither fully informed of these, nor was he able here to give even a limited account of them.

He could, however, refer to facts within the range of common experience, and well known to most engineers who were conversant with questions of fluid dynamics or of naval architecture, which showed sufficiently how inadequate was the mode of investigation adopted in the Paper.

Foremost of these was the fact, that when a plane area moved obliquely through a fluid, the normal pressure was most unequally distributed over the surface of the plane, existing in great excess towards the anterior edge; yet, according to the method relied on in the Paper, the pressure on every unit throughout the surface would be assumed to be the same.

The fact here referred to was the direct consequence and the counterpart of the excess of surface pressure, the genesis of which he described in the earlier part of these remarks. Its existence, as a fact, was at once perceived by any one who, holding a tolerably large plane in his hands, moved it obliquely through water or other fluid, as he would at once find that it exerted a considerable torsional force, indicative of the alleged excess of pressure on its anterior edge. Another simple indication of the same action was seen when a sheet of paper was launched edgeways through the air, and resting on the air as on an inclined plane: it was at once seen that the advancing edge was canted upward, and that the direction of motion was altered by the relative excess of upward pressure under the anterior edge; and a surface would require to be of specialised curvature in relation to its other proportions, in order that it should have an equal supporting pressure throughout, and should thus continue to descend on the air as an inclined plane. The 'boomerang,' indeed, continued to do this approximately, but this was in virtue of its rapid rotation, which

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gave quasi-fixity to its axis; but the peculiar twist which the axis received under the gyroscopic principles which the circumstances called into play here also witnessed to the unequal distribution of the supporting pressure.

Another practical illustration of the same condition was the well-established fact that a rudder, when mounted, as it was in some screw-ships, without the intervention of a stern-post and dead wood between it and the screw, was less powerful in its 'turning' action than if hung to a stern-post in the usual way. In the latter case, when the helm was 'put over,' a considerable lateral pressure was piled up, so to say, on the flat surface of the stern-post, and contributed materially to the whole turning power exerted.

Perhaps a more striking indication of a distribution of fluid pressure on a curved surface, irreconcilable with the Author's hypothesis, was supplied by the *primâ facie* paradoxical curvatures into which sails often arranged themselves under the effect of wind, as was specially noticeable in jibs.

In these sails especially, many sailmakers, for reasons which it would be out of place to enter on here, cut the canvas with an extravagant roundness or convexity of outline on the anterior edge of the triangle (or 'luff of the sail,' as it was called) before roping it; and as the rope was made somewhat shorter than the rectilinear dimension of the side of the triangle, the prominent edge of the convexity became gathered in, so as to form, immediately behind the rope, a narrow tapered belt of slack canvas, which became conspicuously bagged out by the pressure of the wind.

Now it was a most noticeable fact, familiar, doubtless, to all who had studied the 'sit' of sails, that when the vessel which carried such a sail was 'close-hauled,' that was to say, when the wind struck the sail obliquely from ahead, say at an angle of 45° with the line of the keel, the general wind pressure which the reaction of the rest of the sail produced swelled out the 'baggy' belt of canvas, not simply to leeward, but also so much forward that an observer viewing it in a direction at right angles to the vessel's course could see the convexity protruding itself ahead of the bolt-rope, although, from the direction of the wind current as a whole, that part of the sail, when thus protruded by the internal pressure, must experience externally also a considerable direct pressure on its convex or (so to call it) leeward side.

As the vessel was pressed closer to the wind, it was this part of the sail which would first begin to flap or 'lift;' but this would

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not happen until the greater part of the windward surface of the sail was brought so nearly edgeways to the wind, that the flatter or less 'baggy' portions of its surface, were nearly relieved of pressure.

It was but the same phenomenon in another form, when a bag, having its mouth partially opened and presented to the wind, became distended, in a sausage-like form, to a diameter much larger than that of the aperture of its mouth, its shoulders retaining a fully developed convexity, though subject to an external wind pressure, which, though by no means inconsiderable, was yet less forcible than, and was overcome by, the general internal force of distension which the reaction of the bottom and sides maintained within it.

In both these cases the Author's method would not merely fail to appreciate the correct balance of local pressure, but would absolutely ignore, or rather pronounce to be non-existent and impossible, what was, in fact, the conspicuously preponderating pressure.

He need not farther multiply illustrations of the proposition which he had asserted, in opposition to the view of the distribution of surface pressure relied on in the Paper, and he would now turn to the last of the three points to which he proposed to call attention—he meant the misconception which appeared in the Paper as regarded the dynamical relations, or inherent conditions, of the 'slip' of the propeller.

The Author defined 'the slip' as merely "the difference between the speed of the ship through the water, and the speed which she would have in a non-resisting medium under the same power." And he complained of the view expressed by Dr. Main, who, he said, "expressly attributes it to 'the yielding nature of the medium,' as if the law of the equality of action and reaction did not apply to fluid media." He thought that the words quoted did not necessarily convey the interpretation put on them; on the contrary, to say that a yielding medium yielded when force was applied to it did not in any way imply a failure of the relation between action and reaction, any more than it would to say that a gun yielded under the pressure of the exploded powder or elastic gas which propelled the shot, a proposition which precisely illustrated instead of negativing the due relation between action and reaction.

On the other hand, it might be thought that the very paradox of which the Author complained was suggested by his own words, when he spoke of "the speed which the ship would have in a non-

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resisting medium under the same power;" for the exertion of a "power" to keep the ship moving in a non-resisting medium, could such a conjuncture exist, would be precisely "action" without "reaction," though it was certain that the Author had understood his own words in a sense different from that impossible sense which they seemed *primâ facie* to convey. He thought, however, the Author had, at all events, not fully taken account of the dynamical condition which, in fluid propulsion, made "slip," or the equivalent of "slip," an inherent necessity.

The condition he referred to was that when dealing with matter which was free to move, as were the particles of a fluid, if it were wished to exert a forward force by rendering available their reaction, the particles among which the force was exerted must themselves yield, and receive motion from the operation of the force; and not only so, but the amount of motion generated among them in the direction of the backward force had a precise simple and defined relation to the magnitude of the force and the time during which it operated. And that relation was as follows: whatever lateral or transverse derivative motions the particles might receive under the operation of the force, if the momentum generated in the direction in which the force operated during a given interval of time was summed up, that was to say, the components of motion in that direction impressed during the interval on all the particles acted on, that integral momentum would be the exact dynamic equivalent of the operating force.

By dynamic equivalent, he meant as follows: the force of, say a ton, acting during a given interval of time on matter free to move, would invariably generate one and the same amount of momentum in the direction in which it acted, whether the matter it acted on consisted of a single ton of coherent matter, or of a mass greater or less than that in any assignable ratio, or of a multitude of particles acted on either simultaneously or in succession. The momentum thus generated was the dynamic equivalent of the force.

The relation might be expressed as follows: calling the force which acted F, M and V respectively the integrals of the units of mass acted on and of the velocities imparted to them in the direction of the force, during the second, so taken that MV = the momentum thus generated during the second, or (μ); then making all the terms duly commensurate, the expression $F = \frac{MV}{g}$ or $= \frac{\mu}{g}$, where (g) was the force of gravity. Here MV, or μ, was the dynamic equivalent of F.

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Or, if it were desired to adapt the expression specially to the circumstances of most common occurrence, bearing in mind that (g) might be written = 32, when feet and seconds were the denominations used in representing V, and that a cubic foot of salt water weighed 64 lbs., M being counted in cubic feet, it might be said F (in lbs.) = $2MV$ or = 2μ. Thus to maintain (say) 1000 lbs. of propulsive force, the propeller must in each second generate in the surrounding fluid a sternward momentum = $\frac{1000}{2}$, that was to say a momentum equal to that of 500 cubic feet moving with a velocity of 1 foot per second. And in proportion as the mass operated on was larger or smaller, the velocity imparted to the mass would be smaller or larger.

Now assuming that the water acted on by the propeller was stationary when the propeller began to act on it, then since *primâ facie* the propeller must virtually move sternward at least as fast as the water which it pushed, it would seem that the minimum possible slip must have exactly that velocity which it was necessary to impart to the mass of fluid operated on, in order to develop in it the dynamic equivalent of the force; and this proposition, which in fact expressed the highest limitation of slip attainable by the best form of ordinary propeller, pointed instructively to the necessity of always so proportioning the propeller as to make it operate on the largest possible mass of water, so that the sternward velocity which must be imparted to it should be a minimum.

But a theoretically perfect propeller might be imagined, such as would pervadingly operate on each particle of the reacting mass by a direct sternward force, and would impart the motion by a gradual acceleration, as gravity did, rather than by an impulsive blow; and such a propeller would impart the necessary final velocity by operating with only half the mean velocity which it finally attained, as a falling body acquired a velocity of 32 feet per second, though the force which impelled the body, that was to say its own weight, only travelled $\frac{32}{2}$ or 16 feet while imparting that velocity; and since 'the slip' was the mean virtual sternward motion of the propeller, measured as regarded the stationary fluid, it followed that with the theoretically perfect propeller acting on undisturbed water, the mean velocity of the slip was limitable to half the velocity which must be imparted to the mass of fluid operated on.

The result here stated pointed in an interesting and instructive manner to the theoretical possibility of apparent negative slip in a screw-propeller. For the water in which a single central screw operated was not stationary, but consisted of a forward

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current, called the 'wake,' which the transit of the ship had generated.

Part of the forward motion of the particles which surrounded and followed the ship's stern constituted, indeed, what might be termed the "conditions of established motion," belonging to the ship, as, for instance, that of the deadwater behind the stern-post and the 'square tuck' or other heavy lines of the run; or as, again, the converging lines of inward and forward movement, by which the surrounding water refilled the hollow which the transit of the ship had created. These conditions differed in the principle of their constitution from the 'wake' proper, inasmuch as, when once established, they no longer expressed any elements of which the ship's resistance consisted, since they had been established once for all, and either accompanied the ship inertly, as motion resident in unchanged particles, or as motions transmitted from particle to particle along the line of the ship's progress, on principles analogous to those of wave motion, without loss of energy.[1]

Setting aside these passive accompaniments of the progress of the ship, and referring only to the 'wake' proper, there would be seen in this the 'dynamic equivalent' of the ship's resistance, or, what was the same thing, of the propulsive force employed in keeping her in motion; for it represented wholly the constantly renewed forward motion which the friction of her 'skin' against the particles that rubbed against her, and the induced frictional movements of the particles of water "*inter se*" were ever creating in matter previously undisturbed; and it was now well known, that if the divergent waves generated by the ship *in transitu* were excluded (which with a well-formed ship moved at a moderate speed were trivial) these frictionally created movements were responsible for the entire resistance which the ship experienced.

The integral forward momentum, therefore, of the wake generated second by second, which was the exponent of the ship's resistance, must be identical with the integral sternward momentum, simultaneously generated in the 'slip,' which was the exponent of the propulsive force; and since two equal and opposite *momenta*, when brought into opposition, were mutually destructive, it followed that a screw propeller which operated so as to pervade the wake, would precisely neutralise the forward velocity of the wake; as in point of fact visibly happened.

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[1] *Vide* "On the Mathematical Theory of Stream Lines, especially those with four Foci and upwards." By W. J. M. Rankine. Proceedings of the Royal Society of London, vol. xviii., p. 207.

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And if the propeller were made to operate on the wake so far astern of the ship as not to displace the dead water, or interfere with the orderly replacement of the water in the track of the ship (in either of which cases a factitious excess of resistance was developed), and if, moreover, it was assumed to be of the theoretically perfect character already described, then since, as was explained, its mean sternward velocity, or slip through undisturbed water, would be only half of that which it finally imparted to the water, it followed that in neutralising the forward current of the wake and thus maintaining the propulsive force, it would in reality be receding before that current with a mean velocity of retreat equal to half the velocity of the current. That was to say, a theoretically perfect propeller, operating on the wake, quite clear of the ship's stern, might realise an apparent negative slip equal to half the forward mean velocity of the wake at the point where the propeller operated.

Mr. A. Murray, C.B., said that it was evident that a plane surface revolving upon an axis at right angles to it had no propulsive power in it; the action was similar to that of a circular saw, and there would be no propulsion in a plane surface placed in a line with the axis. It was also manifest that between these there must be one angle better than another, which it was the province of mathematicians to determine. The Author referred in the Paper to Mr. Griffiths' propeller, in which the blades were placed upon the circumference of a sphere as a boss, to avoid the nearly flat portion of surface towards the axis. There was no doubt that that, for which the Paper proposed another remedy by altering the angle, was good; but instead of the sphere he preferred, wherever the length of the screw would permit, an egg-shaped boss, the forward part of the egg being made to fit the ship, so that there should be no hollow between the stern-post, for the current of water to go in and come out again over the surface of the sphere, and the after-part of the egg should be equal to the breadth of the stern-post. He thought it better to lessen the diameter of the sphere than to depart from the view he wished to express.

The Author had spoken very clearly on the subject of slip; but he had not made himself sufficiently acquainted with the actual facts of the case, when he said that negative slip was impossible. It really did exist, and the explanation was found in the current of water which followed the ship, produced by her progress through the water. That there should be such a current following the ship was no doubt detrimental, and he looked upon that as evidence of malformation in the after-body of the vessel. If a ship were so

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formed that there was no after-current, and some were so formed, then less work would be required from the engine.

Mr. R. GRIFFITHS exhibited the model of a propeller, and said that what he was going to explain was gained from experience. He objected to most of the theories advanced on the subject of the screw-propeller. In the first place, the ball offered no resistance in going through the water. The water for 3 feet or 4 feet on each side of the screw aperture travelled with the ship, and not only so, but much faster, because it flowed in to fill the space which the ship left. If there were no ball, but the blades were continued through to the centre, they would only churn the water. The model represented a screw 14 feet in diameter, the diameter of the ball being 3 feet 9 inches. Now, if the ball were removed, and the water flowed through the vacant space, the screw would make a greater cone of disturbance, but the smaller the cone the greater was the gain; in fact, it was an object of importance to keep the cone of disturbed water as nearly as possible of the diameter of the screw, which in this case was 14 feet.

In the next place, in that screw as it was there would be slip more or less from 10 to 20 per cent.; but if the same blades were laid to an angle of 20° to 30° towards the ship, there would be negative slip, but the propeller would not give better results. In the 'Flying Fish' the blades were put first at right angles to the screw shaft at a pitch of 20 feet, and then at 16 feet pitch, with the blades inclined towards the ship; the engines made the same number of revolutions and power, but the speed of the ship was no better, although there was apparent negative slip. Slip was a wrong term to use. Suppose a ship were fastened so that it could not move, and the screw set going, a column of water would be driven backwards equal in diameter to that of the screw; so when the ship was going the screw would still drive the same column of water. The same amount of water must be driven through the screw, whether the ship was under way or standing still; consequently it might be called all slip; it was only the resistance got by driving that column through the screw that gave the thrust to the screw shaft for propelling the ship.

If the screw were working in still water, he did not think any form could be better than the true screw when the blades were vertical; but when they were horizontal they were in a very different current. Therefore, unless it was possible to have blades which changed the propelling surface, so as to suit the different currents into which they entered at each revolution, the object aimed at by the Author of the Paper could not be arrived at. It

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was astonishing what a little difference there was between a perfectly flat blade and a true screw. A flat blade would be at its best when in the horizontal position, and a true screw would be at its best in the vertical. He found that screw-propellers with the point of the blade made of a coarser pitch than the root did much better than if made with the pitch the same throughout.

Mr. BRAMWELL said that Mr. Griffiths had made a statement of the results of his actual experience; and those statements ought to be treated with great respect, looking at the undoubted boldness and originality of Mr. Griffiths' views in screw propelling. Twenty-five years ago Engineers made the bosses of screw-propellers as small as possible, and to allow this they went to the length of forging the boss with the blades; at that time, also, the width of the blade at its circumference was commonly equal to the radius. But at that time came Mr. Griffiths, who reversed the whole theory and practice, making the boss one-third the diameter, and the point of the blades narrow, so as not to subtend more than one-sixth of the width they formerly subtended. That was a bold thing to do on reasoning unaided by mathematics.

One proposition of the Author of the Paper was borne out by Mr. Griffiths' practical experience. He understood from the Paper that, instead of making a true screw, the Author would make a screw, whatever the pitch at the circumference was, of less pitch as it approached the shaft; so that in no case would there be more than an angle of 45°, even if the blade were continued to the centre line of the shaft of the propeller. This was what the Author called the semi-helix, and was a screw in which the pitch was gradually lessened as the centre of the shaft was approached. Mr. Griffiths had contended that it was advisable to make a true screw, say of 12 feet pitch, and then so to set the blades (which in Mr. Griffiths' screw were generally adjustable) that they should have 16 feet or 18 feet at the circumference, and 3 feet or 4 feet less where they joined the large boss. In that respect it would be satisfactory to the Author to find that his suggestions agreed with Mr. Griffiths' practical experience; but the Author put it as though the screw of diminishing pitch to the centre was a new thing. Mr. Griffiths and himself had known of it for the last fifteen years, and it was not new then: therefore the Author was wrong in supposing he was the first to point out that modification of the screw.

The Author had spoken of speed in a non-resisting medium; but it appeared to him that if there could be a non-resisting

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medium, any power, if applied long enough, would develop an infinite speed. With respect to slip, that it did exist in a yielding material was obvious; and he thought Mr. Griffiths put it very clearly. The screw was propelling a column of water; whether the ship would move, and how much it would move, would depend upon the resistance offered by the ship. The vessel might be fast to a wharf, and still the screw would propel the column of water. If the ship were unmoored she still propelled the column of water, and the ship made motion. That the matter of slip was not an unimportant one was evident from the circumstance that, when the vessel was moored, the whole of the power of the engine would be absorbed to work the screw and to make slip; and that if it were unmoored, but were attached to something which offered great resistance to motion, then a very large part of the power would be absorbed in mere slip. In other words, it was quite possible to proportion the parts, so well or so ill, that a great or a small proportion of the power should be utilized. After all, this was the true test: what was the proportion of power utilized? Therefore he had contended many years ago, when conducting experiments on Mr. Griffiths' screw, that persons using a screw ought not to complain of the rate of slip; because, supposing with a given consumption of coal one kind of screw would drive a ship faster than another, then, whatever the relative slip might be, the former screw would get over more space with the same consumption of coal, and that was the end to be aimed at.

Slip did and must exist, and good performance might coexist with considerable slip; but, nevertheless, the question of slip was important because its consideration was necessary when proportioning the parts.

The Author further said whenever negative slip occurred it must be due to the action of sails combined with steam, and that seemed to be regarded by him as a sufficient explanation. Supposing in every case of negative slip canvas had been used, that was not an explanation, because, with the decrease in the resistance of the ship, due to the use of canvas, the engines would increase the speed; but it was the fact, as he had himself seen, that negative slip without canvas was a perfectly possible thing, and could be obtained whenever anybody wished to obtain it. Nobody would wish to obtain it, however, because it was occasioned by a larger following current of the ship than other ships had, and that was only got by a greater expenditure of power. But if a ship had a full run, and a screw of large dimensions and of fine pitch, negative slip would exist; and that there might be

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no mistake, he meant a case where, if the screw was working say 6 knots, the ship was going say 7 knots an hour, and that without canvas. The Author, however, said that was an impossibility, and that wherever negative slip happened it was due to the use of canvas. From his own observation he could say that was not so. Mr. Froude had very properly said it would not do to take one part of a propeller blade only and consider that part by itself; it must be taken as a part of the whole. This fact the Author had overlooked; and as a consequence the Paper afforded no instruction whatever as to what should be the number or area or form of the blades.

Mr. W. AIRY said the Author had not fixed upon the precise helical surface best adapted for screw-propellers, but he had determined the family of surfaces which answered the conditions. It was stated in the Paper that, "This surface has a property in common with the ordinary helix. If it be intersected by a cylinder concentric with the shaft, the spiral thread or trace on the surface of this cylinder will make a constant angle with all lines on the cylinder drawn parallel to its axis; but the essential difference was that a plane through the axis cuts this surface in a curve, instead of in a straight line." He would observe that both these conditions were fulfilled by the family of developable surfaces, such as he had found best adapted for Archimedean screws for lifting water.[1] He had not himself investigated the general equation to these surfaces, but he had laid a model on the table to explain his meaning; and he would like to ask the Author whether that family of surfaces was comprised in the general equation which he had obtained for the screw-propeller. With regard to the curve in which the developable spiral surfaces were cut by a plane through the axis of the core, it would be something of the nature of a hyperbola; for it had an asymptote, to which the curve constantly tended, and which it never reached, and it sprang from the surface of the core at right angles. The shape of the spiral surface which the Author had arrived at as best adapted for screw propellers appeared to be due to the same principles as those which governed the weathering of windmill sails. These were spiral surfaces very nearly parallel to the plane of rotation at the extremities of the sails, and considerably inclined to the plane of rotation at the parts towards the axis. As the conditions were theoretically very similar, it would seem likely that the spiral surface which was found to be best for windmill sails would give a clue to the spiral surface which would be best for screw-propellers.

[1] *Ante*, p. 1.

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Mr. C. G. GUMPEL said the Author had overlooked the physical phenomena involved in the screw propeller. He would give an illustration. No mathematics would ever prove that it was the pressure of the atmosphere that drove the water up the pump. This was the result of a known physical phenomenon, but mathematics would determine how high the water would rise in the pump, according to the pressure of the atmosphere; when the practical man would step in, and show that the physical impossibility of producing an airtight piston would make it doubtful whether the water could be drawn up the pump to the theoretical height. In a similar way he looked upon screw propulsion as a physical phenomenon, and the question was how far water would offer resistance in giving a fulcrum for propulsion. It seemed extraordinary that the Author ignored that one point which had always been the stumbling-block in the question of propelling ships, viz., the yielding nature of the water; he, in fact, declared that the water was of too unyielding a nature. Now, the yielding nature of the water was the only guide in designing a screw-propeller of maximum efficiency, so far as influenced by form of blade, as other elements of the propeller could only be determined by the conditions of special cases. He would illustrate his meaning by the following experiments:—Take a board of about 10 square feet at the end of a stick, dip it into water, and apply a constant pressure against the water in a direction normal to the surface of the board. The water would be found to yield upon the continuous pressure. Upon taking a board of only 1 square foot, and applying the same amount of pressure in a similar manner against the water, but for a short space of time only, the water would be found less yielding. This evidently proved that time was an important factor in estimating the yielding of the water; that the longer each particle of water was pressed upon the readier it would be to yield. If this test was applied to a paddle-wheel, it would be found that the paddle-float, acting, while immersed, constantly on the same quantity of water, caused it to yield readily, so that the entering float was the really only efficient one, by its concussion against the undisturbed water. The screw-propeller, owing to its oblique action, encountered constantly fresh particles of water, comparatively at rest, and acted on them for a more or less short time only. This would explain why a long screw was inefficient. The screw-blade, by acting in a constantly changing circular direction, had the natural tendency of driving the water before it in a tangential direction, away from the axis. The particles of water which were taken up by the leading edge of the blade near the axis would leave

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the blade of a long screw near the circumference, hence there would be formed a conical hollow and a tendency to a vacuum behind it, which would act as a drag on the vessel instead of propelling her ahead. A short screw, for the same reason, would show better results, since it constantly took up fresh particles of water, which left the blade again after their inertia had been used as a fulcrum, and before they had commenced yielding to a more or less considerable extent. He would call attention to the experiments made in the 'Dwarf,' in 1844, in which the length of the screw was reduced from 3 feet 6 inches to 1 foot 3 inches, when a maximum effect was gained.

With regard to the question: What was the best form of screw-blade to obtain the best fulcrum in the water? not only must the blade be narrow, but it must be such a blade as would counteract the centrifugal action, and would direct the particles of water towards the axis instead of tangentially; in fact, it must be a curved blade, curved in the direction of the screw's rotation, instead of being placed radially; it would then be found that the water would be driven aft in a cylindrical column.

A screw of such a form had lately been tried in one of the Government vessels, and was proved to possess an advantage of 10 per cent. as compared with Griffiths' screw; as 400 H.P. less out of 4,000 H.P. were required for the same speed; and he had no doubt that it was the only screw that would be found to meet all expectations of efficiency.

He was tempted to refer to some remarks of Mr. Murray's and Mr. Griffiths' to show with what different views the phenomena of screw propulsion were looked upon. When speaking of negative slip, Mr. Murray showed that the rational way of explaining this phenomenon was, by taking into account, in the progress of the screw, the current of water which followed the ship with a certain velocity, in which current the screw-propeller found its fulcrum. And he very rightly observed that, by dragging such a following current behind the ship, great loss of power was experienced. Mr. Griffiths, when explaining the efficiency of his screw-propeller, used this same phenomenon to show that power was gained by the special form of the boss. The vacuum which this boss left behind it, he said, produced a tendency in the surrounding water to close in upon the centre, and thereby prevented centrifugal action. This surely could not but be a loss of power. Besides which, the better plan to prevent centrifugal action of the propeller was by forming the blades in the manner already pointed out. It was a question, however,

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whether the large boss was necessary to prevent the churning near the axis; since the method alluded to by Mr. Griffiths, of making the blade of a correspondingly less pitch near the axis than at the circumference, would prevent this evil. Take, for example, a screw of 20 feet pitch with an allowance of 10 per cent. slip, then the pitch of the blade near the axis would be made 18 feet, so that, when the vessel was going at a fair speed, no churning of the water would occur. He considered that experimental enquiry into the physical phenomena of the question at issue would be more serviceable than mathematical investigations to determine a screw of maximum efficiency.

Mr. W. AIRY said that the spiral exhibited was one of the models with which he illustrated his Paper on the Archimedean screw for lifting water. The spiral threads of this model were visibly of the nature of developable spiral surfaces. The Author had noticed two properties of the surface of maximum work which he had taken as particularizing that surface. The first was that it resembled an ordinary helix in making a constant angle with lines on an intersecting cylinder drawn parallel to its axis; the second was that a plane containing the axis cut the surface in a curve and not in a straight line. The combination of these properties was undoubtedly a peculiar feature, and they were both satisfied by the developable spiral surface of the model.

Mr. BRAMWELL remarked that the pitch of the screw near to the wooden axis of the model exhibited by Mr. Airy was the same as at the circumference; but the Author of the Paper recommended that the pitch of the screw towards the axis should be less than the pitch towards the circumference.

He did not understand Mr. Airy to say that the developable screw would do that which the Author contended, viz., make a screw of less pitch at the centre than at the circumference. The Author said, "This it is which enables a moderate angle to be obtained close to the shaft, where the crank exerts the greatest power, and the blade, not clearing the water, does not cause the vibration observable in the common helix, while its hold upon the dead water is a clear gain in power." From this it was clear that the Author considered the best screw of all was where the pitch was less at the centre than at the outside.

The mode in which the Author proposed to proportion the angles at different circumferences of the propeller to the angle determined to be employed at some one circumference in that propeller might be thus stated in popular language.

Let "a" be one of the circumferences in the propeller, and by

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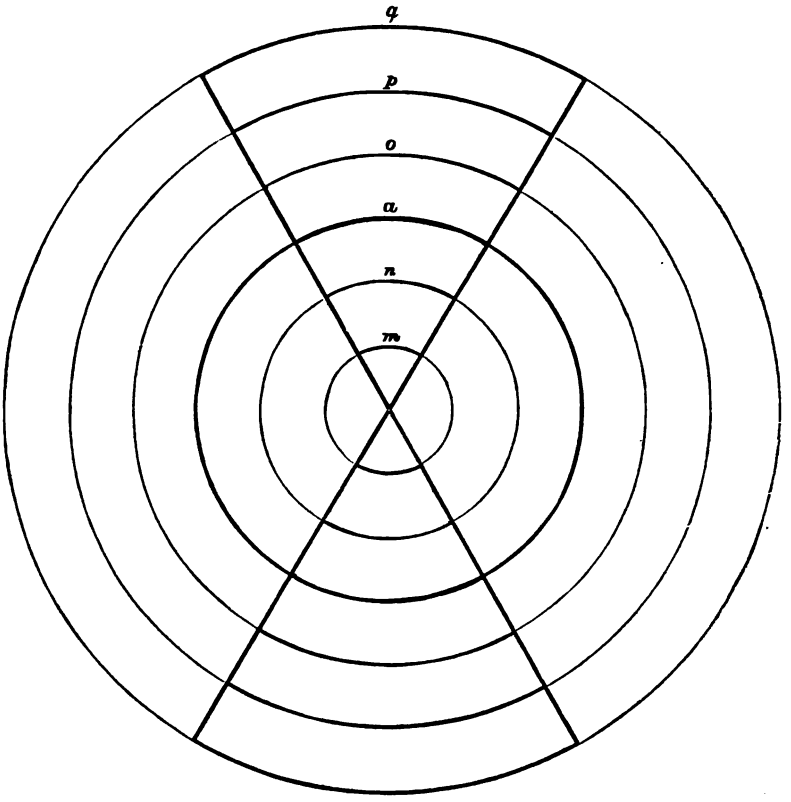
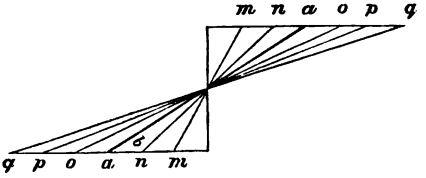
preference let this circumference, "*a*," be half that of the propeller (Fig. 4).

Fig. 4.



Face view of an ordinary true Screw two-bladed Propeller, having an angle of 30° at the circumference *a*, with a uniform pitch of 0·90726, the diameter of the propeller being unity.

Fig. 5.



End view of one of the blades of Fig. 4 (but with the segments laid out into straight lines), showing the angle *b* of 30° for the circumference *a*, and the varying angles for the other circumferences.

Let "*b*," Fig. 5, be the determined angle to be made at that circumference by the face of the propeller with a plane at right angles to the axis, say for the sake of illustration 30°. Then

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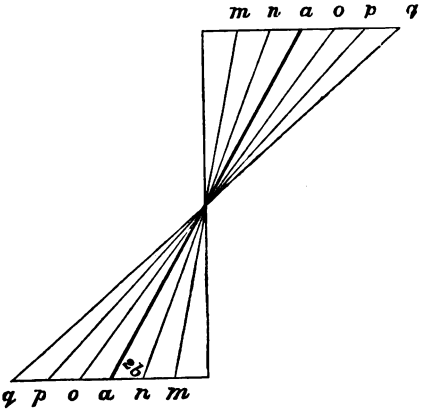
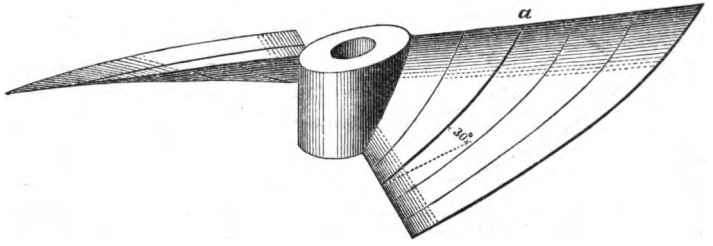
double the angle "*b*," and with that double angle, 60°, set out a true screw, Fig. 6, that was to say, a screw of equal pitch for all circumferences of the propeller.

Fig. 6.



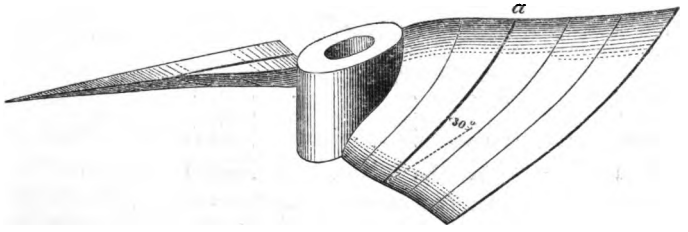
End view of one of the blades of a true Screw-Propeller, as in Figs. 4 and 5, but with the angle *b* doubled (= 60°)—showing also the varying angles for the other circumferences, with a uniform pitch of 2·72199, the diameter of the propeller being unity. In Figs. 4, 5, and 6 the angle at the centre is 90°.

Fig. 7.



Ordinary two-bladed true Screw-Propeller.

Fig. 8.



Two-bladed Screw-Propeller proposed by the Author.

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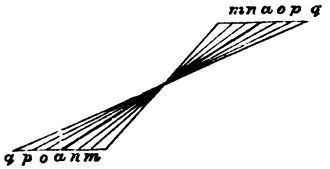
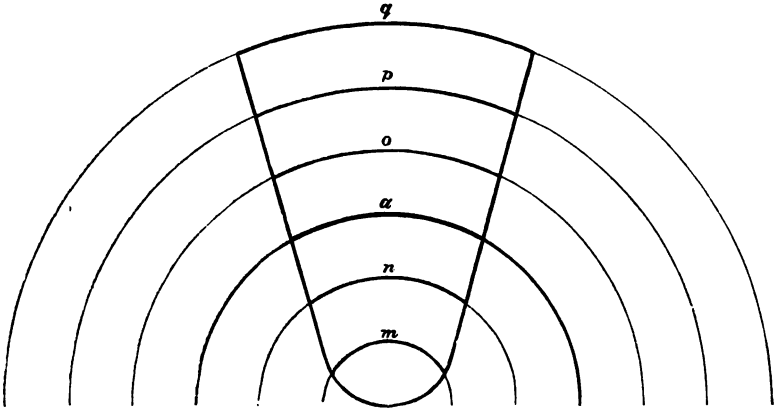
Then to make the actual propeller recommended by the Author, his 'semi-helix,' bisect the angle made at any circumference, *m*, *n*, *o*, *p*, *q*, between the face of this true screw and a line at right angles to the axis, and a propeller surface corresponding with these bisections would give the semi-helix, Fig. 9.

Fig. 9.



End view of one of the blades of the Author's proposed Propeller, formed by bisecting the different angles of Fig. 6, and using these new angles to give the twist of the blades, which is no longer a true screw, but one of a pitch at the circumference of *a* = the pitch of Figs. 4 and 5, and of a pitch increasing from the circumference *a* towards the outer circumference, and decreasing from the circumference *a* towards the axis. See also Fig. 10.

Fig. 10.



Showing the curved form of the edges of the blades when they are not true screws, but are of a twist in accordance with Fig. 9.

In Figs. 9 and 10 the angle at the centre is 45°.

In Fig. 10 the pitch at	*q*	is	1·168
" "	*p*	is	1·11
" "	*o*	is	1·028
" the determined pitch at	*a*	is	0·90726
" the pitch at	*n*	is	0·7194
" "	*m*	is	0·4319

Such a propeller would fulfil the Author's conditions.

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1st. At any circumference, *m*, *n*, *o*, *p*, *q*, its face would be at an unvarying angle.

2nd. Whatever the angle determined on at the circumference "*a*," that at the axis would be 45°.

3rd. If the propeller were cut by a pair of planes at right angles to the axis, the boundary lines of the blades of the propeller would not be straight radial lines as in the true screw, Fig. 4, but would be curved as in Fig. 10.

Admiral E. P. HALSTED said that nothing had yet succeeded in producing better effects in absolute propulsion than the original true screw, which was identical in mechanical principle and formation, whether worked for driving ships through the water, or used as a fastening to the hinge of a door. That true screw, he believed, had never yet been surpassed in its effect of maximum work; and he had never known an authenticated instance in which it had been excelled. The difference of form, at the extreme edge, between the true screw, and that of Woodcroft's, of increasing pitch, was so small that either screw could be cast in the same mould, the difference amounting to only the fractional part of an inch. In relative work however there was this remarkable difference—that when tried in the same ship under similar circumstances, the increasing pitch, used with its fine edge foremost, gave nearly similar results, with similar power, to the true screw; but when used with the increasing pitch foremost there was a material increase of power developed by a greater number of revolutions, with a considerable decrease of effect. The result of turning the increasing pitch end for end was that ships could not 'back astern' so effectively with the Woodcroft's screw as with the true screw; but in 'going ahead,' throughout all the trials of those days, all modifications of the true screw gave favourable results between the one and the other, precisely as the variation in pitch and other conditions came nearer and nearer to those of the screw with a uniform pitch. This matter was closely experimented on and discussed at the time, and he did not think much more was known about it now. At that time, too, the practical effect of negative slip was known, and in some remarkable cases no repetition of trials could alter it. Notice was then also taken, and stress laid on the fact, that with ships of any form, or of any size, going through the water at any speed, there was, and still must be, what came to be called in screw propulsion 'a following current,' which filled up the hole made by the ship's motion, precisely as fast as it was made, and without which there must be a hole, or furrow, or re-

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duced level of the water, astern of her. In that 'following current' was recognised the solution of what the screw did in contrast with the paddle, and why the ship, when under sail, could not over-run the screw; because, whether driven by sails, or by steam, or even pulled by horse power, there always was this current, in which the screw in its true position in the 'dead wood' found its ever-following fulcrum. It was not determined then, any more than now, what were the details of action by which the water following the ship was operated upon by the screw; nor the various directions and degrees whence were derived the power of the current as regarded its following the varying motion of the ship. In those days, however, there were apertures, up which the screw was raised out of water, that the propeller should cause no obstruction to the ship while sailing; and on thrusting an oar down these apertures at the foremost corners when the screw was at work, it was often found very difficult to hold it, not from its being dragged behind or astern, but from its being powerfully dragged forward by the strength of the stream rushing under the bottom from aft. There were still these grave mysteries about this matter, to solve which, he thought, there must be a great deal more practical observation, before the final facts could be arrived at on which to found a formula.

Admiral G. Evans remarked that in 1837, having carefully examined Mr. Smith's screw-propeller, then fitted to a boat lying in the West India Docks, he was much surprised at its simplicity, and saw that it was far superior to the paddle; and only required, for the protection of the screw from ropes and weeds, a prolongion of the keel and a false, or outer, stern-post, on which the rudder could be hung, and the screw to be placed in the space between the two stern-posts.

Lord Sligo and Lord Western got up a company, and built the 'Archimedes,' screw-steamer, and in 1839 he made a report to the Admiralty on the performance of that vessel.[1] Having been employed since then in other departments, he had no further experience of the working of the screw, or he would have endeavoured to get a trial of Mr. John Brett's plan of using a four-bladed screw when steaming against heavy head-winds, and altering it to a two-bladed screw when under sail or during fine weather. By that plan, the steam now blown away would be used on the four blades, to great advantage, during the prevalence of strong head-winds, while the speed of the vessel was necessarily slow.

[1] *Vide* The Nautical Magazine and Naval Chronicle for 1852, p. 676.

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Mr. G. H. PHIPPS remarked, through the Secretary, that he did not consider mathematics usefully applicable to any subject abounding to such an extent with unknown laws and quantities as the action of the screw-propeller. Nearly all the attempts made for the improvement of the screw-propeller, from its first useful application, by Mr. F. P. Smith, Assoc. Inst. C.E., up to the present time, were of an empirical character. For instance, at first a large portion of the spiral equal to, at least, one entire revolution was considered necessary. Then the increasing pitch suggested by Mr. Woodcroft and others began to be a favourite idea, until, as time went on, $\frac{1}{6}$ of a revolution was admitted to be a sufficient length of the spiral, and was generally adopted in the Royal Navy.

The French found, indeed, that the screw-blades might be still further diminished with advantage; the only limit being apparently to leave sufficient substance of metal for the necessary strength. The increasing pitch was generally admitted to be of little or no advantage, as indeed must be apparent when the blades were so much reduced in depth.

When the screw-propeller superseded the paddle-wheel, it was naturally reasoned, that as to obtain more and more power, with the latter, it was necessary to use larger and larger paddle-blades, so, in substituting the screw, an analogous increase of surface acting upon the water must be obtained. The discovery that no such increase was required led him to an entirely new set of ideas, and, in his opinion, gave the key to an explanation of the great propelling power given out by that instrument.

Every small unit of the contact surface of a screw propeller in motion might be considered as a plane surface, moved through the water in a direction at a very acute angle with its own plane; and when this was the case, it was well known that the pressure against the side of such plane vastly exceeded that arrived at by the law of the square of the sine exceeding it all the more as the angle became more acute. He had particularly pressed this fact in a Paper read before the Institution in 1864,[1] as explanatory of the impossibility of comparing the resistance of bodies in water when differing considerably in magnitude, and which were not similar bodies; as, if the draught of water were identical in two cases, and the angles of entrance also the same, both these bodies would encounter equal resistances of the kind above referred to, *i.e.*, above the law of the square of the sine, while all the other resistances might differ largely.[2] Another point as to the resistance of plane surfaces

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxiii., p. 221.

[2] *Vide* Transactions of the Society of Engineers for 1868, p. 220.

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consisted in the fact, that whilst moving either at right angles, or at some smaller, but still considerable angle, to themselves, the law of the resistance being as the square of the sine was nearly correct, and the depth of immersion caused no appreciable difference; but with plane surfaces, very little inclined to their direction of motion, the amount of immersion constituted an important element of the resistance.

An explanation of the cause of the great comparative resistance to the motion of very slightly inclined planes might in his opinion be found in the circumstance, that all the water displaced by them was so displaced, laterally, to their line of motion; and so all the particles of water in advance of the plane were left nearly in a state of absolute rest. The plane, consequently, when coming into contact with particle after particle, could only displace them by raising up the whole of the column of water above them. Suppose a plane 1 foot square, immersed to a mean depth of 9 feet, inclined at such an angle to the direction of its motion as to give a sine of 3 inches, and moved at a speed of 18 feet per second, it would have to put into motion, in one second of time, $18 \times 9 \times 62{\cdot}5 =$ 10,125 lbs. of water, at a speed of $\frac{18}{4} = 4{\cdot}5$ feet per second, equal to $\frac{\text{gravity}}{7{\cdot}1} = \frac{10125}{7{\cdot}1} = 1{,}426$ lbs. pressure against the side of the plane. By the law of the square of the sine, the pressure against the side would be,

$$\frac{\frac{18^2}{8} \times 62{\cdot}5}{4} = \frac{2{\cdot}25^2 \times 62{\cdot}5}{4} = 79 \text{ lbs.}$$

It would thus be seen that on this law the resistance was $\frac{10125}{79} = 128{\cdot}16$th part only of the former. Great as this difference was, however, it would in effect be doubled if a smaller plane, 12 inches by 6 inches only, had been taken, as on the first mode of calculation, the resistance would not be diminished by the reduction in area; whereas, on the second, it would be reduced one-half.

Going now to the Paper under discussion, he contended that as so important a matter as the above increase in the law of resistance of inclined planes was left untouched, involving as it did the important fact of the equal efficiency of short planes with long ones, all the elaborate logic of mathematical reasoning used by the Author must needs fail in arriving at the truth.

The paragraph he would first touch upon was where, speaking

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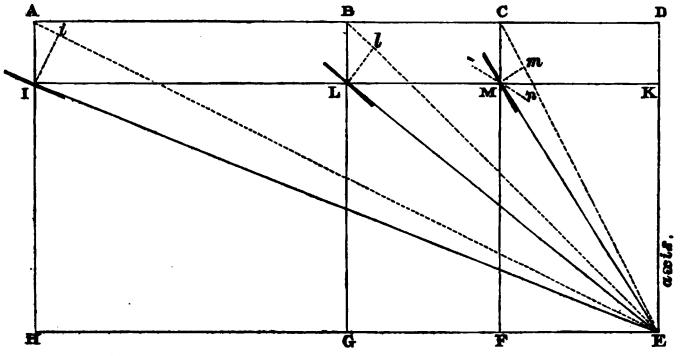
of the properties of the peculiar helix recommended, the Author said, "This it is which enables a moderate angle to be obtained close to the shaft, where the crank exerts the greatest power; and the blade, not churning the water, does not cause the vibration observable in the common helix; while its hold upon the dead water is a clear gain in power." Now he considered that a considerable fallacy was involved in these observations. In the first place, he was far from admitting that the acuteness of the angle of the ordinary helix near the shaft was a disadvantage. He believed that, theoretically, every portion of the ordinary screw-blade gave out a thrust commensurate with the power employed in working that portion; that if the term 'churning' meant water through which any portion of the screw actually passed, at right angles to itself, which he called slip, this would be greater with the helix described than with the ordinary one; and, lastly, that 'vibration' had nothing to do with the 'churning,' but arose from the shocks experienced by the entrance of the screw-blades into water, either at rest, or in various angles and directions of motion, in the course of each revolution of the shaft. In Fig. 11 was represented the development of a screw with ordinary

Fig. 11.



helix, in which A H and D E represented the pitch, A D its circumference at the periphery, B D = ½, and C D = ¼ of A D. If there were no slip, the ship and screw travelling at the same rate, the course through the water of the different units of the blade, I, L, and M, would be on the lines E A, E B, and E C; but allowing ⅛th of A H for slip, the actual courses became E I, E L, and E M. Now, first, as regarded the relation of the power absorbed by each unit of contact surface to the work done by it in propelling.

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It was a recognised fact, in estimating the power of any metallic screw, working in a nut, that provided the length of the lever applied were the same, no difference of effect would arise, except friction, which was here disregarded, from the largeness or smallness of the diameter of the screw; and, consequently, any power applied to the lever would respectively raise corresponding weights through the distances F C, G B, and H A; but, in consequence of the slip, in all cases equal, and corresponding to A I, $\frac{1}{3}$th of A H, $\frac{2}{3}$ths only of the power applied on each unit was realized. There was, therefore, no greater proportion of power lost on a unit M, near the axis, than upon I, remote from it; and, as the distance M *m* was less than either L *l*, or I *i*, there was less churning of the water at the interior than at the exterior circles. It also appeared that, if the angle of the unit of surface at M could be altered according to the Author's views to, say M *n*, such alteration would increase, rather than diminish, the churning of that unit, and that if altered more than to bring it in line with M E, instead of propelling it would actually obstruct.

As to the Author's objection to the term slip, he was quite at a loss to conceive how any word could better convey the meaning intended. The blade of an oar was let fall into the water, a strain was put upon the oar, the blade shifted backward a certain distance, and it surely slipped through the water that distance. Again, the use of the hypothesis of a vessel moving in a "non-resisting medium" was, to him, perfectly unintelligible, since no power could be required to propel such a vessel. And, finally, as regarded negative slip, he could not admit the correctness of the Author's assertion that "it can only arise from the joint action of steam and sails," as it was well known often to exist under steam alone, and could well be traced to the effect of water in motion, carried along by the vessel itself, and that, all the more in proportion as the after lines of the vessel were deficient in fineness.

Sir F. C. KNOWLES, in reply, observed that there was very little room for discussion between him and Mr. Froude, when Mr. Froude objected *in toto* to the hitherto accepted principle of resistance to motion in fluid media, on which the whole of his investigation was founded. He could only, as to this principle, give a direct demonstration of the principle itself, which demonstration, as far as he could judge, assumed nothing beyond the well-established laws of force and motion, and the ordinary mechanical constitution of incompressible fluids. He, however, would point out that the apparent discrepancies, from the principle of the resistance being "as the square of the velocity multiplied by

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the square of the sine of the angle of inclination of a plane to its line of motion," might more probably be attributable to the inaccuracy of the constant of resistance, and more frequently, in the cases of bodies moved with very high velocities, in elastic and compressible media, which might greatly modify the phenomena and the apparent law of resistance. It had been contended, in the course of the discussion, that the application of mathematical reasoning in the treatment of such subjects was wholly inadequate and out of place; and perhaps it was not agreeable to be so tied down to measure and number, as it was apt to check the imagination. But it was rather too late in the day to object to the treatment of questions touching material force and motion by mathematical analysis, when it had been so long and so successfully applied to phenomena infinitely more varied, complicated, and delicate than those presented by any possible question in the mechanics and hydrostatics of engineering. Mr. W. Airy, however, had fairly entered into the spirit of his method, and appreciated its results, and the illustration from the 'weathering' of windmill sails was as appropriate as it was conclusive. He was disposed to think these surfaces were comprised in his group. Mr. Andrew Murray also seized the very essence of the problem and its importance. He might have put this in a popular form as follows: "Take, for every point within the limits of the blade's dimensions, an indefinitely small plane surface, or facet, at the end of an arm or radius from the axis, and place each such facet in the position to give the greatest possible propulsion in the direction of the keel, by rotation round the axis; then unite from point to point all these minute facets, supposing them to cohere and to become one polyhedral surface. Lastly, pass from the discontinuous to its limit, the continuous arrangement of the system, by supposing the facets infinitely small and infinite in number, and there would result a surface comprised in his solution of the problem. The surface so obtained was of course only one type of the family which answered the conditions." He did not here mean his method of solution, but the kind of result obtained by it; the method itself, by means of the abstruse relations of quantities in or about the state bordering upon or attaining a maximum or a minimum value, determining the form required. Mr. Froude overlooked one important consideration, that, even admitting the law of resistance to be defective, as he asserted it to be, if he should be the fortunate person to discover the true law, the method employed in the Paper was the only one which could be effectually applied by his law

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to the determination of the best form of propeller. There was a choice between a certain mode of selection, and endless trials, with endless disappointment and expense. He saw no obstacle to following out, by a correct mathematical analysis of the case, the motion of every particle of water acted upon either directly by the screw, or indirectly by means of the ship's form. The great difficulty was to take account of the secondary disturbances, due to the waves of water acted upon by statical pressure, which waves rushed in laterally to fill up the void left from moment to moment by the ship in her course. He could not omit to remark that Mr. Froude proposed no definite principle to replace the present one. He suggested only one ($v \cdot \sin \theta$) to throw doubts upon its application at oblique angles, in favour of some other which he had 'in petto.' Of this latter he did not give even an approximate expression, or it might at once be put to the test of some simple practical application susceptible of a conclusive trial. It would occupy too much time to analyse all the cases which Mr. Froude put in illustration of his adverse views of the law, or rule, of the square, but he saw in them quite enough to be convinced that all the views enunciated might be reconciled with the law in question, or the apparent discrepancies from it assigned to independent disturbing forces inseparable, or at least not yet separated, from the conditions of the experiments upon resistance. He might remark that in gunnery, where accuracy in calculating the trajectory of a projectile was of the greatest importance, no artillerist had ever proposed to abandon the principle of the square of the velocity for that of the simple velocity; for example, though the trajectory in the latter case could be readily calculated, and in the former (unfortunately) could not be calculated at all except by a laborious approximation.

He had already referred to the constant of resistance, as k in $k v^2$, and he would have it borne in mind, in dealing with these somewhat vague objections, that in many experiments upon resistance rough surfaces were used; so that it would be difficult to say whether the simple sensible perpendiculars to the apparently plane surface were the real normals. For example, in a wavy surface the real normals would be only those on the crests and on the hollows of the waves in direct motion; and, in oblique motion, only those between the crest and the hollow. Thus only a portion of the particles would, in either case, be in the conditions of motion which were the supposed data of the problem. There was an analogous case in light reflected very obliquely to an ordinary table, where a bright image was ob-

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tained: none but the faintest, if any, being perceptible at higher angles. And the cause of this difference was correctly assigned to the perfect reflection of light from the prominences, unmixed with the scattered rays from the hollows. Now, it was only required to suppose these ridges and hollows of the planes, which were used in the experiments on resistance, to be large in proportion to the diameter of the molecules of water, to be sure that such a physical conformation must largely interfere with the uniformity of the results. This, then, would be no reason for abandoning the law of the square, *à priori* the most probable, but simply a motive for employing none but the most highly-polished substances, and in every case of change of material to modify the constant of the formula. The perfect conformity of the law, or principle of the square, to the known laws of force and motion, raised a high, *à priori*, probability in favour of its correctness, and a no less probability was created by the simple explanations of all apparent deviations from the law which were afforded by the above given physical imperfections of the planes employed to test the truth of the hypothesis.

No one seemed to have understood the result at which he had arrived on the subject of 'slip' of the screw, although the views upon this matter of Mr. Griffiths and Sir Francis Knowles were virtually identical. It was asked whether an oar did not 'slip' back in the water. Without inquiring whether the term was appropriate, he answered to the meaning, certainly it did, but in doing so it communicated motion to matter; to the water in one direction, and to the boat in the other; so that, if the mass of water was m, and its velocity v, the mass of the boat being M and its velocity V, while the mass of the water pushed ahead was m' and its velocity v', then $m\,v = \mathrm{M}\,\mathrm{V} + m'\,v'$. But the slip in question was 'slip;' not a source, but a supposed loss of power; for, what other could it be, when the advance of the whole system, ship and screw together, was less than the advance of the screw in a nut with the same number of turns per minute? *Pro tanto* the screw might as well revolve in the air! This was not his view of the matter, for he said there was no loss of power in any way, or direction; nor was there any 'slip'—a word used only to mask an erroneous theory. Consider, irrespectively of any theoretical views upon the subject, how 'slip' was computed. The number of turns of the screw working alone, and in a nut per minute, gave the potential speed in advance in direction of the keel. From this was deducted the actual speed of the screw and ship together, in the same direction; and this difference, divided by the former

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speed, was the ratio called 'slip.' Now, what was this potential speed of a screw working in a nut under the same power of the engine? Was there any deduction from it for either friction or resistance of any kind? None! Then, if not, could the speed of the point be other than the unresisted speed *in vacuo*? Take the formulæ of Professor Main, viz.:—

w = angular velocity of the screw in rotation;
b = $a \tan \alpha$ (a = the radius, α the angle of the screw);
then $w\,b$ = the potential speed in advance of a point in the edge of the blade;
and $w\,b$ put = u, let v = speed of ship and screw;
then $\frac{u - v}{u}$ = the 'slip' of the screw.

Now, in these elements there was no symbol which took into account friction, or resistance, of any kind. And it followed that all such sources of retardation were supposed to be non-existent, and that this speed of the screw in a nut was its unresisted speed *in vacuo*. Now, in this condition put before it a ship on a perfectly smooth plane. The ship under the power of the engine would attain the same speed as that of the screw connected with it, and the two would go on together. But, what was this but the motion of the ship in a non-resisting medium? And could the ratio of the above difference, $\frac{u - v}{u}$, be any other than the measure of the degree in which the medium resisted the motion, or, as he had properly called it, was unyielding to it? It was a mere affair of convertible terms! Let the question be viewed in another light, which seemed to afford a decisive criterion. If there was any inherent defect in the medium which led to loss in the form of what was called 'slip,' it ought to be constant for the same screw, and to follow it as its shadow when transferred to another ship with a smaller or a larger midship section; but the algebraical expression for 'slip' contained H, the area of the effective midship section, and the 'slip' must therefore vary from one vessel to another, in all cases save where it was in a constant ratio to β A—an exceptional case. His interpretation of the formulæ showed that this was so, because the larger or smaller the effective midship section, the greater or smaller must be the resistance to motion, respectively, and the difference between the speed of the ship at its best, and the potential speed in a non-resisting medium, must be greater or less accordingly. Again, it could not be by accident that the algebraical expression was the

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same in both cases. As to 'negative' slip, it meant, according to the explanation of his critics, that in the last resort the screw made a greater number of turns per minute and advanced with the ship more than if it were working in a nut; in fact, that it was positively accelerated by motion derived from its own power —in other words, perpetual motion. It might fairly be asked whether there was any but an insignificant "following of water" due to friction between the ship's hull and the water, a motion speedily subdued by the mass of inert water into which she was momentarily intruded? The real movement of water abaft, due to statical pressure, was transverse from opposite sides, and these lateral waves rushing to fill up the void astern, were compounded with the motion of the column driven through the screw (their limiting lines depending on the form of the stern) in such a manner that they must either wholly destroy each other, or partially so, leaving a part to conspire with the sternward column. The former condition might so far deaden the water astern as to increase the reaction, or thrust, of the screw, and thus, by increasing its efficiency, appear to diminish slip, the speed of ship and screw increasing accordingly. He should call it diminishing the relative resistance by an increase of speed in the ship; but, for the rest, as 'slip' could never be = 0, *à fortiori*, it could never be negative, by any motion derived from its own power, whether direct or indirect. He wished it to be understood that in his mention of Mr. Griffiths' moveable screw blades he had no intention to depreciate the merits of his invention; on the contrary, he thought it highly ingenious and important; and whatever might be the latent advantages of a screw of his own type, he was ready to admit that experience alone could show which was the most advantageous as a propelling machine, some well-selected form of his own or that of Mr. Griffiths.

He had tried the simple power ($v \cdot \sin \theta$) in his method, but it gave no result beyond the ordinary family of helicoids.

It would be well to engage the British Association to institute a course of experiments upon resistance with highly-polished surfaces, and to compare the results obtained with those given by ordinary planes.

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April 25, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

No. 1,305.—"Further Experiments on the Strength of Portland Cement." By JOHN GRANT, M. Inst. C.E.

IN a previous Paper, Session 1865-6,[1] the Author stated that "further experiments are desirable, on the strength of adhesion between bricks and cement, under varying circumstances; on the limit to the increase of strength with age; on the relative strength of concrete made with various proportions of cement and ballast," &c. The experiments described in this communication have been tried with the view of throwing some additional light upon these points; and though no one can feel more than the Author their fragmentary and incomplete character, he has, after long hesitation, decided to lay them before the Institution. If they serve no other purpose, their incompleteness may point out to those interested in the subject, and have time for further investigation, the direction which their inquiries might advantageously take, and the large field yet open for their labours.

Before describing the new series of experiments it may be useful to review some of the points in the previous Paper. Among these were "the limit to the increase of strength with age." Tables XVIII., XXIV., XXV., and XXIX. were intended to illustrate this point, and the five years which have since elapsed have to some extent afforded the desired opportunity.

Thus in Table XVIII. were given the results of one hundred and sixty out of three hundred experiments intended to extend over ten years, and made with Portland cement weighing 123 lbs. to the imperial bushel. It was shown that neat cement which at seven days broke at 817·1 lbs. increased gradually in strength till at two years it bore a tensile strain of 1324·9 lbs., and at three years of 1314·4 lbs., or practically the same. By the extended Table in the Appendix, it will be perceived that the maximum was attained at two years, the further results being 1312·6 lbs., 1306 lbs., 1308 lbs., and 1327·3 lbs. at four, five, six, and seven

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxv., p. 79.

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years respectively. With cement and sand in equal proportions the tendency to increase in strength continued; that which at seven days broke at 353·2 lbs., or 42 per cent. of neat cement; at one month at 452·5 lbs., or nearly 50 per cent. of neat cement; at two years at 790·3 lbs., or 60 per cent. of neat cement; bore strains of 818·1 lbs., 821 lbs., 819·5 lbs., and 863·6 lbs., at four, five, six, and seven years respectively. The results at four, five, and six years are practically the same, being 62·6 per cent. of neat cement, and at seven years 65 per cent.

Table XXIV. gives the strength of Roman cement at various stages from seven days to seven years. The results do not uniformly and regularly increase. It was shown that this cement bore a strain of 201·83 lbs. at seven days, of 376·8 lbs. at six months, and of 323·8 lbs. at twelve months. The further experiments give the following results: 438 lbs., 450·8 lbs., 512·6 lbs., 466·9 lbs., 466·6 lbs., and 553·2 lbs., at two, three, four, five, six, and seven years respectively. The differences between the minimum and the maximum are very great, and confirm the conclusion, that this kind of cement is not nearly so uniform in strength as Portland cement, and that though about two-thirds of the price, it is only about one-third of the strength, and therefore double the cost of Portland cement measured by strength.

Table XXV. relates to another Roman cement, and brings down the experiments six years later. Cement which at seven days broke with a tensile strain of 202 lbs., and attained to its maximum 643·1 lbs. at twelve months, broke at 546·3 lbs., 603·8 lbs., 632·2 lbs., 627·4 lbs., 666·4 lbs., and 708·7 lbs., at two, three, four, five, six, and seven years respectively.

Table XXIX. refers to Medina cement, which at seven days bore strains of 92·1 lbs. (1st series), and 211 lbs. (2nd series), attained a maximum strength of 476·9 lbs. at twelve months, and bore only 276 lbs. at two years. At three, four, five, and six years the strains were 275·5 lbs., 287·8 lbs., 307 lbs., and 365 lbs. respectively. This series of experiments is to be continued to seven, eight, nine, and ten years.

As a preliminary to the further experiments hereafter described, upwards of two hundred were made to ascertain if any improvement could be devised in the form of mould previously used. The results of these are given in Table I. of Appendix. Form No. 1 is that which was adopted at first (January, 1859), because it was found in use both in France and in England. Form No. 2 shows the same mould with the inner angles rounded off. In

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Form No. 3 the outer angles and shoulders were also rounded off. This form and the other shapes which follow were found to have a tendency to slip in the clips when under tension. Forms Nos. 4, 5, and 6 were like dowels of different lengths, but with the same sectional area at the neck as the preceding, viz., 1½ inch by 1½ inch = 2¼ square inches. Forms Nos. 7, 8, and 9 were like the three preceding, but with a breaking area of 2 inches by 2 inches = 4 square inches. Form No. 10 is that shown on Plate 3 of the original Paper, and has the same area as Forms Nos. 7, 8, and 9. Twenty moulds were made of each kind; ten were broken at seven days, and ten at thirty days, all having been kept in water. Forms 2 and 3 gave the highest and almost identical results. It was therefore presumed that No. 2, which is the form of mould shown in Plate 4 of the original Paper, was of all the least subject to error and irregularity; and this form has been used in the tests and experiments described in this Paper, except where otherwise stated, and is the standard constantly employed by the officers of the Metropolitan Board of Works. Perhaps the lower results given by the moulds Nos. 4—9 were due to the wedge-shaped ends opening the breaking-clips when the strain was applied. The later investigations of Mr. Bramwell, "On the Influence of Form on Strength,"[1] would lead to the preference of this form to those marked Nos. 1, 2, 3, or 10. Further experiments are being made on this point, and means taken to prevent the clips from opening.

On another point a large number of experiments were made, viz., as to the best mode of avoiding any distortion or departure from the line of strain. The plan originally adopted is shown on Plate 2, and a modification on Plate 4, of the former Paper. It was found, however, that the altered moulds frequently broke at the holes in each end of the specimen instead of at the neck. The original clips were therefore reverted to, but in combination with knife-edges in the eye and pin at each end.

The next step was to establish the conditions to be observed in the following new series of experiments:—

A. On the strength of Portland cement tested by tensile strain at different periods, from one day to twelve months.

B. On the adhesion between bricks cemented with Portland cement and lime mortars, tested by tensile strain at the end of twelve months.

C. On the strength of Portland cement bricks, neat, and with

[1] *Vide* Report of British Association, 1869, p. 422.

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different proportions of sand, tested at the end of twelve months by compression in a hydraulic press.

D. On concrete blocks of different proportions of Portland cement and lime, with gravel, sand, and other materials, tested at the end of twelve months by compression.

For the purpose of these experiments the directions given were:—

"Take about 40 bushels of Portland cement weighing not less than 112 lbs. per bushel; sift the whole of it through a sieve with twenty holes to the lineal inch.

"Note the proportionate quantity which will not pass through this sieve. Fill each bushel measure from a moveable hopper placed at an uniform height of 2 feet above the mouth of the measure, and weigh each 'striked' bushel. Afterwards put the whole quantity together and turn it over many times to insure the thorough admixture of the cement. Use the same cement for the several varieties of experiments.

"Ascertain the quantity of cement required to fill a mould, and the quantity of water required to bring the cement to the consistency of paste.

"In filling a mould with neat cement, use always the same proportion (by weight) of cement and of water.

"Get 40 bushels of clean sharp sand, wash and afterwards dry it; take the weight per bushel, and in making tests with different proportions of cement and sand weigh the proportions of each—thus, to insure equal quantities of Portland cement and sand, take, say $\frac{1}{10}$ part of a bushel of cement (by weight) and $\frac{1}{10}$ part of a bushel of sand (by weight), and mix the two together for an experiment of 1 to 1.

"Use scales and weights, the latter adapted to show decimal parts of a lb. instead of ounces.

Experiments. *Series* A.

"On the strength of Portland cement tested by tensile strain at different periods, and with different proportions of sand. Kept in water.

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Time in Moulds. Ten Moulds each.	Mixed by Hand.	Ground in a Mortar-Mill for Thirty Minutes.
	Broke at lbs.	Broke at lbs.
1 day . . .	,,	,,
2 days . . .	,,	,,
3 ,, . .	,,	,,
4 ,, . .	,,	,,
5 ,, . .	,,	,,
6 ,, . .	,,	,,
7 ,, . .	,,	,,
1 month . .	,,	,,
2 months . .	,,	,,
3 ,, . .	,,	,,
4 ,, . .	,,	,,
5 ,, . .	,,	,,
6 ,, . .	,,	,,
7 ,, . .	,,	,,
8 ,, . .	,,	,,
9 ,, . .	,,	,,
10 ,, . .	,,	,,
11 ,, . .	,,	,,
12 ,, . .	,,	,,
	Total, 190 moulds.	Total, 190 moulds.

Series B.

Kinds of Bricks to be used.

1. Gault clay. Pressed.
2. Do. Wire cut.
3. Do. Perforated.
4. Suffolk (brimstones).
5. Picked stocks or shippers.
6. Fareham red.
7. Blue Staffordshire, pressed, with frog.
8. Do. do. rough, without frog.

"Before using the bricks:—

"1st. Take their dimensions in inches and tenths; give area of bed, area of edge, and cubic contents.

"2nd. Weight—dry.

"3rd. Do. after being soaked twenty-four hours.

"4th. Strength by compression in a hydraulic press.

"Put together blocks, each composed of four bricks, making the joints ¼th of an inch thick, viz.—

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i. 10 of the gault clay, pressed, with neat cement.
ii. „ with Portland cement and sand 1 to 1
iii. „ „ „ 1 to 2
iv. „ „ „ 1 to 3
v. „ „ „ 1 to 4
vi. „ „ „ 1 to 5
vii. 10 with lias lime (specially burnt and ground forty minutes in an edge-stone mortar-mill) and sand, in the proportion of 1 to 2.
viii. 10 with ground Dorking lime-mortar.
ix. 10 with chalk lime-mortar.

"Make the same number of blocks of each of the other kinds of bricks.

Series C.

"Make—

10 Bricks (9 in. by 4½ in. by 3 in.) of Portland cement neat.
„ „ Portland cement and sand 1 to 1
„ „ „ 1 to 2
„ „ „ 1 to 3
„ „ „ 1 to 4
„ „ „ 1 to 5
„ „ „ 1 to 6
„ „ „ 1 to 7
„ „ „ 1 to 8
„ „ „ 1 to 9
„ „ „ 1 to 10

"Put half of each kind in water and keep the other half out of water; test at end of twelve months by compression.

"Take weight dry, and same wet.

"Make a similar series of cement bricks, compressing them in moulds either by hydraulic press or by ramming, and test as above.

Series D.

"Concrete blocks, 12 inches by 12 inches by 12 inches, and 6 inches by 6 inches by 6 inches. To be tested at end of twelve months.

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"One of each to be kept in the open air, and one immersed in water all the time.

4 to be made of 1 cement to 1 ballast.

"	"	2	"
"	"	3	"
"	"	4	"
"	"	5	"
"	"	6	"
"	"	7	"
"	"	8	"
"	"	9	"
"	"	10	"

"Make two of those above by compressing the materials in layers of 1 inch thick, either by rammers or in a hydraulic press.

"After setting, put one of each in water as before. Note the quantity and weight of the materials composing the two kinds of blocks.

"Make—

4 of 1 Portland cement to 6 ground pottery.

"	"	8	"
"	"	10	"

"One of each to be kept in air, and one immersed in water. Two of each kind to be formed by compressing the materials as before.

"Make twelve more of same proportions, using ground slag.

"Make twelve more of ground bottle glass.

"Make twelve of ground flints; the chalk on the surface of the flints to be washed off by diluted muriatic acid.

"Make twelve more of broken or ground granite and twelve of broken or ground Portland stone."

In accordance with these instructions 38 bushels of Portland cement were procured; the gross weight being 4,300 lbs. 11 oz., or 113·176 lbs. per bushel. When sifted through a sieve of four hundred holes per square inch this was reduced to 4,201 lbs. 4 oz., or 110·56 lbs. per bushel. About 36 lbs. were afterwards rubbed through the sieve; 34 lbs. would not pass, and there was a loss of 29 lbs. The sifted cement used in the following experiments, except when otherwise stated, weighed therefore 110·56 lbs. per 'striked' bushel. A certain quantity of cement weighing 19·2 lbs. was sifted through a sieve of four hundred holes per square inch,

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18 lbs. passing through and 1·2 lb. or $\frac{1}{15}$th being left. Five moulds made of the sifted material neat were placed in water, and at the end of seven days broke at 959·4 lbs. = 427 lbs. per square inch. Five more moulds were made of the same cement unsifted, and under the same conditions broke at 842 lbs. = 375 lbs. per square inch. The gain by sifting was therefore about 14 per cent.

The following are the weights per bushel and per cubic foot of the materials used in the new series of experiments:—

Materials.	Weight of 1 Bushel.	Weight of 1 Cubic Foot.
	lbs.	lbs.
Portland cement	110·56	86·375
Sand and ballast	123·40	96·400
Portland stone	98·00	76·560
Broken granite	116·00	90·625
„ pottery	113·00	88·280
„ slag	107·00	83·594
„ flints	126·00	98·440
„ glass	120·00	93·750

Table 2, Series A, gives the strength of neat Portland cement used throughout these experiments at different periods from one day to twelve months; first, mixed by hand, and next, ground in a mortar-mill for thirty minutes. At the end of a month that which was ground in a mill had less than three-fourths the strength of that which was mixed by hand. The maximum strength of that mixed by hand seems to have been attained at five months, and that ground in a mortar mill at one month, the greatest strength of the former being nearly double that of the latter. The strength of that which was mixed by hand was maintained, while that which was ground in a mortar-mill declined, from the maximum in each case to the end of the experiments. This result was probably due partly to the process of crystallization, or setting, having been interrupted by the continued agitation, and partly to the destruction by attrition of the angular form of the particles.

Table 3, Series B, on the tensile strain required to separate bricks cemented together with Portland cement and lime mortars, would require to be greatly extended before any very trustworthy deductions could be made from them. The area from 36 inches to 40 inches is not large, and yet there are considerable mechanical difficulties in making this class of experiments with any machine which combines the necessary strength and delicacy.

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The pressed gault bricks show the lowest amount of adhesiveness; partly because of their smooth surface, and partly because in making them some oily matter is used for lubricating the dies of the press through which they are passed before being burnt. In the case of the perforated gault bricks the cement-mortar seems to act as dowels, and the results are consequently high. The Suffolk and the Fareham red bricks, which each absorb about a pound of water per brick, adhere much better than the Staffordshire, which are not absorbent. This shows the importance of thoroughly soaking bricks which are to be put together with cement, as dry bricks deprive the cement-mortar of the moisture which is necessary for its setting.

Table 4, Series C, on the strength of Portland cement bricks tested by crushing, is, so far as it goes, instructive. As a rule, and as might be expected, the denser the brick, the greater its strength. When the cement is in proportion to the sand less than 1 to 2 or 1 to 3, those dried in air bear a greater pressure than those kept for twelve months in water. This would lead to the inference, that when the quantity of cement is small, bricks or blocks of concrete should be kept some time out of water, and be allowed to harden before being used.

Contrasting the strength of these concrete bricks with the different clay bricks as given in the first column of the Table 3, it will be seen that down to the proportion of 5 to 1 the former compare favourably. Thus, bricks made of neat cement bore a pressure equal to that of Staffordshire blue bricks or of best Fareham red bricks. Cement bricks made in proportions of from 2 to 1 of cement to 5 to 1 are equal to the picked clay bricks of the first six varieties in Table 3. It is further evident that if these concrete bricks were more compressed their strength would be greatly increased. The compressed specimens in the table were only from 3 to 9 per cent. denser than those not compressed. Further experiments on the relation of density to strength are most desirable.

The tables referring to the D series show the strength of concrete blocks, or cubes, made with Portland cement, mixed with various materials in different proportions and crushed after being kept a year, half of them in air and half in water. The general deductions from these tables are, that the blocks made with the largest proportion of cement to ballast are the strongest—the strength being nearly in proportion to the quantity of cement; that it is desirable to spend no more time than is absolutely neces-

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sary to effect a thorough admixture of the cement with the sand and gravel; and that the compressed blocks are apparently stronger than the uncompressed blocks in a larger proportion than their difference in density.

In this series Tables 4, 5, and 6 give the strength of 12-inch cubes of concrete made with ballast, Portland stone, broken granite, pottery, slag, flints, and glass, mixed with Portland cement in the proportions of 6 to 1, 8 to 1, and 10 to 1, and compressed. Half were kept in water for twelve months. Tables 7 to 12 give the strength of 6-inch cubes of concrete made of the same proportions, and with the same materials, half of them compressed and half uncompressed. Some of these were kept in air and some in water for twelve months. Tables 13 to 18 exhibit the results of the six preceding tables in another form.

The most prominent result of the D series of experiments is that concrete made of broken stone or broken pottery is much stronger than that made of gravel. This is no doubt due partly to the greater proportion of cement absorbed in the latter case in cementing the finer particles of sand, and partly to the want of angularity in the gravel. Compression and an increase in the proportion of cement alike increase strength. In making concrete bricks or blocks of moderate size compression might be applied with advantage; but with large masses of concrete it would be difficult to do so without running the risk of interrupting the process of crystallization or setting, which commences immediately on the application of moisture. The cost of labour so applied would therefore be better employed in a larger admixture of cement. For the same reason that absorbent bricks should be thoroughly soaked with water before being used, the broken stones, bricks, or other materials used in making concrete should be saturated with water before the cement is applied.

Different illustrations of the use of Portland cement in the construction of sewers for the Main Drainage of London, and of the Albert or Southern Embankment of the river Thames, are shown in Plate 15.[1] It will be observed that in some cases it has only been used as a foundation, or as a backing for brickwork; while in other instances sewers, 4 feet 6 inches high by 3 feet wide, of concrete, have been lined with brickwork 4½ inches thick (Fig. 7).

[1] In the Appendix, page 304, will be found "Extracts from the Specifications for Sewers made of Portland Cement Concrete and Concrete lined with Brickwork."

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Again, some sewers have been formed entirely of concrete, in the proportion of 1 of cement to 6 of sand (Figs. 8, 9, and 10). There have been constructed in the metropolis, south of the Thames, 5,440 lineal feet of concrete sewers, lined with 4½-inch brickwork, and 6,800 lineal feet (about 1¼ mile) of sewers entirely of concrete. The details of these will be found in Table 5 in the Appendix. The cost of this concrete is less than half that of brickwork, but if rendered inside with cement it is about the same as if lined with half brick—perhaps the cheapest form of sewer combining strength with soundness. Sewers and culverts of almost any size might be made on this principle. Sewers of concrete, not rendered inside, though somewhat cheaper, have one practical disadvantage in busy thoroughfares, inasmuch as they require a longer length of centering, on account of the slow setting of the concrete, and it is therefore necessary that about double the length of trench should be open at one time. Fig. 8 shows the invert-mould for forming the lower half of concrete sewers. Iron ribs are set up 12 feet apart, and narrow laggings are inserted between them. The concrete is put in and rammed behind the latter. The centre is planed smooth, and if the internal face is not to be rendered the centre is greased to prevent the concrete adhering. Where the arch is not to be rendered, fine concrete, composed of sand instead of gravel, is put next to the centre. The cost of the concrete sewer, 4 feet by 2 feet 8 inches (Fig. 9), was 10*s.* per lineal foot, exclusive of excavation. Under the same contract a brick sewer, of the same size, 9 inches thick (Fig. 6), cost 16*s.* 6*d.* per lineal foot. The concrete sewer, 7 feet 1 inch in diameter (Fig. 10), cost per lineal foot, irrespective of earthwork, 16*s.*; but inclusive of earthwork, side entrances, junctions, &c., it was about 23*s.* per lineal foot. This sewer was in some respects exceptional, inasmuch as it consisted of little more than an arch over a previously existing invert; the lower half was however rendered with cement and sand, in equal proportions, 1 inch thick. Everything being taken into consideration, the most economical combination is 4½ inches of brickwork in cement and the rest in concrete. Another sewer, 9 feet 6 inches by 9 feet 6 inches, to be constructed of concrete with a lining of 4½-inch brickwork in cement, is shown in Fig. 11.

In the construction of the Albert, or Southern Embankment of the river Thames (Fig. 12), it was originally intended to form the wall of brickwork, with a granite facing; but after about a fourth part of the work had been executed, 14,335 cubic yards of Portland cement concrete, made in the proportion of 6 to 1,

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g. 3.

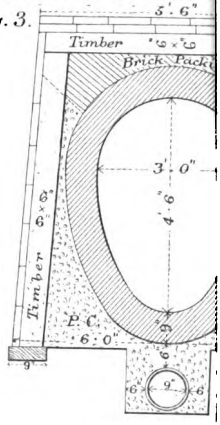


Fig. 6.

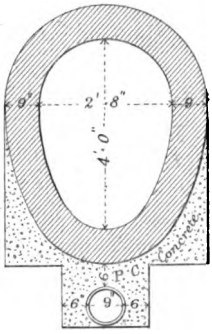
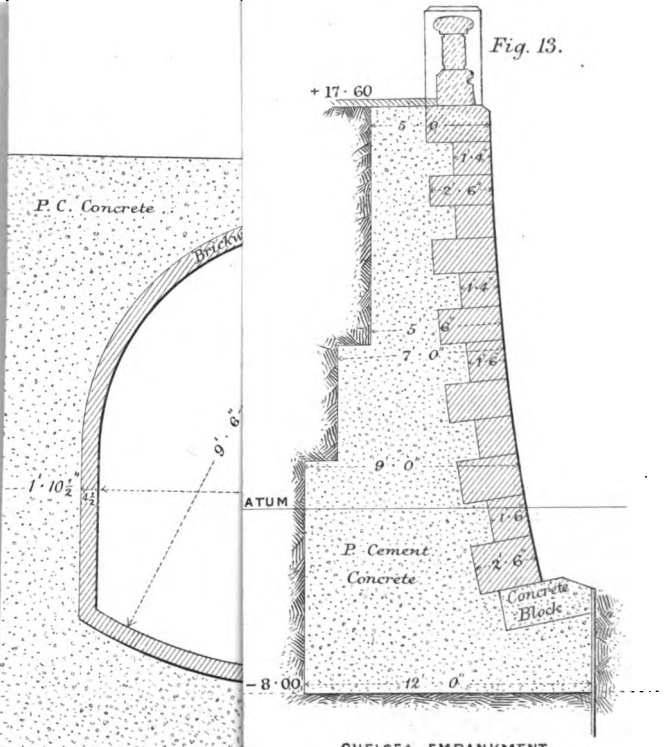


Fig. 13.



CHELSEA EMBANKMENT

PORTLAND CEMENT CONCRETE, FACED WITH GRANITE.

Length 4200 feet.

CONCRETE

Scale for Fi

5 4 3 2

Scale for Figs 12 & 13, 8 Feet — 1 Inch.

10 5 0 10 feet

KELL, BROS LITH^RS CASTLE S^T HOLBORN E.C.

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at 11*s.* per cubic yard, were substituted for an equal quantity of brickwork at 30*s.* per cubic yard. In the No. 3 contract of the Victoria Embankment about 15,000 cubic yards of Portland cement concrete were used instead of brickwork. The Chelsea Embankment Wall, soon to be commenced, will be formed of about 25,000 cubic yards of cement concrete, faced with about 5,000 cubic yards of granite (Fig. 13).

From the experience already gained in the use of Portland cement concrete, there would seem to be hardly any limit to the purposes to which it may be applied. It is gradually being brought into use in the construction of dwelling-houses in different parts of the country, and there is no doubt it will be still more extensively employed in the construction of docks, piers, breakwaters, and other massive engineering works. Mr. Joseph Mitchell, M. Inst. C.E., has tried Portland cement concrete, made of broken granite and cement, and laid on about 6 inches thick, at Inverness, Edinburgh, and in London, as a substitute for ordinary metalling for the surfaces of the streets of towns.

Many experiments have been made in the manufacture of bricks of different proportions of Portland cement and sand. As these can be made equal, if not superior, both in strength and appearance, to most kinds of clay bricks, it is difficult to understand why they are not manufactured as a substitute for clay bricks in localities where the latter are dear. Probably, however, it is only a question of time. Where concrete can be used in a mass it is cheaper than when used in the form of blocks, and still cheaper than in the form of bricks. Each of these modes has its advantages, according to the nature of the work.

In France it has been long used in the form of 'Bétons Agglomérés.' In 1867 a number of arches were formed with it by M. Coignet, under the steps leading from Westminster Bridge to the Albert Embankment, and also about 40 feet of sewer, 4 feet by 2 feet by 8 inches, in the Camberwell Road. Similar arches and sewers were constructed of Portland cement concrete, and the general result was that the Portland cement concrete was both stronger and cheaper than the béton.

In the construction of a sewer at Dulwich, where a cement weighing 118 lbs. per bushel was used, considerable quantities of milky looking fluid exuded from the joints for a few weeks after the work was completed. This was probably the result of an excessive proportion of chalk having been used in the manufacture of the cement. With the view of ascertaining whether or not the cement deteriorated with age, the tests shown in Table 6 were made,

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first with neat cement, then with cement and sand in equal proportions. It was satisfactorily ascertained that in both cases the cement continued to harden during the three months over which the experiments extended. The neat cement at seven days bore a tensile strain of 805·2 lbs., and gradually increased in strength to 1,239 lbs. at three months. The mortar composed of cement and sand in equal proportions stood a strain of 275·8 lbs. at the end of a week, and 538·6 lbs. at three months. The strength of the latter mixture increased from 34 per cent. of the neat to 43·4 per cent.; and had the experiments been extended over a year or more, analogy would lead to the conclusion that this proportion would have continued to increase.

Table 7 gives the strength of 589,217 bushels of Portland cement used during the last five years on various works south of the Thames. The cement has borne an average tensile strain at the end of a week of 806·63 lbs., equal to 358·5 lbs. per square inch; being an improvement on that reported five years ago of 200 lbs. on the breaking area of 2¼ square inches, or 89 lbs. per square inch. It is satisfactory to know that the quality has not only been maintained, but has continued to improve. The strength at the end of thirty days of 37,200 bushels of the same cement, as ascertained by eleven hundred and eighty tests, averaged 1,024 lbs., equal to 455 lbs. per square inch, showing an average of 234 lbs., or 30 per cent. over the cement tested at seven days, which broke at 790 lbs. Wherever the nature of the work will admit of it, tests at the end of a month will be found more satisfactory than if made earlier, as heavy cements, though the strongest eventually, are the slowest to set.

The standard originally specified, 400 lbs., on 2¼ square inches, was soon afterwards raised to 500 lbs., or 222 lbs. per square inch. This has since been increased to 350 lbs. per square inch, or 787 lbs. on the breaking area at seven days (Table 8). A standard of 450 lbs. per square inch at thirty days would be about an equivalent practical test to specify. For the purpose of comparison the same sectional area at the breaking point (2·25 inches) has been retained, though various experiments, already described, have been made as to different forms.

Further experience has confirmed the earlier conclusions, that the strength of Portland cement increases with its specific gravity, its more perfect pulverization, and its thorough admixture with the minimum quantity of water in forming mortar. It has also shown that to effect perfect cohesion, it is of the utmost importance that the bricks to be cemented together should be thoroughly

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saturated with water. Dry bricks absorb moisture from the cement and destroy it.

Heavy cement, weighing 123 lbs. a bushel, like that referred to in Table XVIII., takes about two years to attain to its maximum strength used neat: the admixture of sand not only reduces its strength, but prolongs the period during which its strength increases—in other words, by the admixture of sand or gravel, cement-mortar, or concrete, sets less rapidly than neat cement.

Roman cement, though from its quick-setting property very valuable for many purposes, deteriorates by exposure to the air before use, and is about double the cost of Portland cement if measured by strength.

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APPENDIX.

TABLE XVIII., in Appendix to Paper 1865–6, extended to Seven Years.[1]

Results of Experiments with Portland Cement, weighing 123 lbs. to the Imperial Bushel, mixed neat, and with an equal proportion of clean Thames Sand, showing the Breaking Weight on a Sectional Area of 2·25 Square Inches. The original form of mould is shown in Plate 2, vol. XXV., and in Table I., No. 1, of the Appendix to the present Paper.

These form parts of a Series intended to extend over 10 Years. The whole of the Specimens were kept in Water from the time of their being made till the time of Testing.

AGE.	Neat Cement. Average Breaking Test of 10 Experiments.	1 of Cement to 1 of Sand. Average Breaking Test of 10 Experiments.
	lbs.	lbs.
7 Days	817·1	353·2
1 Month	935·8	452·5
3 Months	1055·9	547·5
6 ditto	1176·6	640·3
9 ditto	1219·5	692·4
12 ditto	1229·7	716·6
2 Years	1324·9	790·3
3 ditto	1314·4	784·7
4 ditto	**1312·6**	**818·1**
5 ditto	**1306·0**	**821·0**
6 ditto	**1308·0**	**819·5**
7 ditto	**1327·3**	**863·6**

TABLE XXIV., in Appendix to Paper 1865–6, extended to Seven Years.

Results of Experiments with Roman Cement, weighing 80 lbs. to the Imperial Bushel. Manufactured by Messrs. COLES, SHADBOLT, and Co. Sectional area, 2·25 Square Inches. Original Mould.

TIME KEPT IMMERSED IN WATER.	Neat Cement. Minimum Breaking Test.	Maximum Breaking Test.	Average Breaking Test.
	lbs.	lbs.	lbs.
7 Days	180	213	201·83
14 ditto	130	215	168·00
21 ditto	200	253	221·14
1 Month	173	283	243·33
3 Months	238	400	329·60
6 ditto	252	408	376·80
9 ditto	78	306	167·80
12 ditto	137	428	323·80
2 Years	**377**	**472**	**438·00**
3 ditto	**425**	**462**	**450·80**
4 ditto	**454**	**574**	**512·60**
5 ditto	**442**	**494**	**466·90**
6 ditto	**447**	**487**	**466·60**
7 ditto	**488**	**620**	**553·20**

[1] In Tables XVIII., XXIV., XXV., and XXIX., the darker type refers to the later experiments.

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TABLE XXV., in Appendix to Paper 1865–6, extended to Seven Years.

Results of Experiments with Roman Cement. Manufactured by Messrs. J. B. WHITE and BROTHERS. Sectional Area, 2·25 Square Inches. Original Mould.

TIME KEPT IMMERSED IN WATER.	Neat Cement.		
	Minimum Breaking Test.	Maximum Breaking Test.	Average Breaking Test.
	lbs.	lbs.	lbs.
7 Days	170	240	202·0
14 ditto	160	190	173·0
21 ditto	170	205	186·5
1 Month	246	291	260·3
3 Months	307	344	322·5
6 ditto	442	502	472·7
9 ditto	313	520	471·1
12 ditto	596	680	643·1
2 Years	**577**	**610**	**546·3**
3 ditto	**522**	**647**	**603·8**
4 ditto	**600**	**658**	**632·2**
5 ditto	**582**	**662**	**627·4**
6 ditto	**603**	**711**	**666·4**
7 ditto	**646**	**780**	**708·7**

TABLE XXIX., in Appendix to Paper 1865–6, extended to Six Years.

Results of Experiments with Medina Cement. Manufactured by Messrs. FRANCIS BROTHERS, 1864. Sectional Area, 2·25 Square Inches. Original mould.

TIME KEPT IMMERSED IN WATER.	Neat Cement.		
	Minimum Breaking Test.	Maximum Breaking Test.	Average Breaking Test.
	lbs.	lbs.	lbs.
7 Days	83	100	92·1
ditto (2nd series)	195	235	211·0
14 Days	238	335	303·4
21 ditto	274	332	298·0
1 Month	210	346	306·0
3 Months	420	468	448·8
6 ditto	376	438	412·4
9 ditto	438	507	457·2
12 ditto	456	527	476·9
2 Years	235	328	276·0
3 ditto	**200**	**342**	**275·5**
4 ditto	**236**	**430**	**287·8**
5 ditto	**245**	**395**	**307·0**
6 ditto	**309**	**475**	**365·0**

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TABLE 1.

Results of Experiments on different forms of Moulds. Weight of Portland Cement used, 114 lbs. per Bushel. October and November, 1866.

No.	Form.	Dimensions at Breaking Point.	At 7 Days.			At 30 Days.		
			Breaking Weight.	Breaking Weight per Square Inch.	Average per Square Inch.	Breaking Weight.	Breaking Weight per Square Inch.	Average per Square Inch.
1		1½ × 1½	778·1	345·8	..	1010·7	449·2	
2		ditto	1034·7	459·9	..	1151·6	511·8	
3		ditto	1064·3	473·0	..	1145·4	509·0	
4		ditto	918·9	408·4	..	969·0	430·66	
5		ditto	924·0	410·6	..	967·0	429·8	422
6		ditto	860·0	382·2	..	912·1	405·4	
7		2 × 2	1199·2	300·0		1354·7	338·7	
8		ditto	1172·6	293·0	297	1315·1	328·8	337
9		ditto	1190·5	297·6		1371·2	342·8	
10		ditto	1286·3	321·6	..	1460·3	365·0	

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TABLE 2. SERIES A.

EXPERIMENTS on Cement mixed by Hand and in a Mortar Mill. The Form of Mould is shown in Table 1, No. 2. Sectional Area, 2·25 Square Inches. 1868. Average of Ten Experiments in each case.

Cement, 110·56 lbs. to the Bushel. Mixed by hand.		Cement, 110·56 lbs. to the Bushel. Ground in a Mortar Mill for 30 Minutes.	
1 day	362·1	1 day	341·1
2 days	509·8	2 days	388·1
3 ,,	698·3	3 ,,	478·3
4 ,,	765·0	4 ,,	512·6
5 ,,	794·4	5 ,,	563·7
6 ,,	829·8	6 ,,	604·1
7 ,,	897·5	7 ,,	613·2
1 month	1096·3	1 month	755·8
2 months	1215·0	2 months	557·7
3 ,,	1220·0	3 ,,	611·6
4 ,,	1422·9	4 ,,	521·9
5 ,,	1435·6	5 ,,	504·8
6 ,,	1424·9	6 ,,	538·2
7 ,,	1357·8	7 ,,	451·4
8 ,,	1360·1	8 ,,	637·8
9 ,,	1372·4	9 ,,	496·6—3 moulds broke.
10 ,,	1381·1	10 ,,	Nil—all moulds broke.
11 ,,	1387·1	11 ,,	256·4—2 moulds broke.
12 ,,	1396·5	12 ,,	443·2

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SERIES … TESTS.[1]

… required … bricks cemented to …

DESCRIPTION	Dimensions in …			Cubical Content
	Average length	Average breadth	Average thickness	
	ins.	ins.	ins.	ins.
… clay, …	…	…	24·06	99·25
	…	…	24·75	102·09
	…	39·375	23·625	103·48
	…	40·5	23·625	106·31
	…	…	23·125	97·4…
	…	4·125	2·75	36·09	24·06	99·2…

[1] … of this Series may be consulted at the Institution.

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TABLE 3. SERIES B. SUMMARY OF TESTS.

of 4 with Portland Cement and Lime Mortars, at end of 12 Months.

Weight in lbs.		Tensile Strain in lbs.									Set in Air or Water.
		Portland Cement and Sand.						Lime and Sand.			
Dry.	Wet.	Neat.	1 to 1.	2 to 1.	3 to 1.	4 to 1.	5 to 1.	1 to 2 Blue Lias.	1 to 2 Dorking.	1 to 2 Chalk.	
5·13	6·47	1631·4	1576·4	871·	988·2	735·2	Broke in drilling.	1448·0	1520·5	638·7	Air.
		1673·4	1642·	1708·	1264·	750·3		..	..	..	Water.
5·86	6·85	2440·	1558·3	Not set.	1037·	791·	772·	Not set.	Not set.	679·4	Air.
		1679·	1408·3	Not set.	Not set.	Not set.	Not set.	..	..	..	Water.
4·95	5·76	3898·8	2982·	2231·6	1663·2	1088·6	812·6	1763·0	1349·3	547·0	Air.
		3011·	2721·	1834·4	1561·4	933·2	641·	..	..	..	Water.
6·18	7·14	3275·2	2346·	2247·	2030·6	1930 6	1337·	2372·0	1788·2	428·6	Air.
		3231·	2484·	2238·	1934·4	1869·	1154·2	..	..	..	Water.
5·	5·57	2816·8	2264·	1571·8	1516·2	825·8	792·6	1280·6	1479·3	423·8	Air.
		3445·	2455·	2009·	1580·8	1259·6	845·6	..	..	..	Water.
6·55	7·52	4538·	2990·	2064·	1499·	1236·	1040·	1919·0	1743·0	449·0	Air.
		4449·	2237·	1917·	1128·	1145·	653·	..	..	..	Water.
7·82	7·90	2563·4	2008·	1844·	1535·	1183·	752·	1102·7	1330·6	Not set.	Air.
		2749·	1329·	1304·	1053·	1016·	718·	..	..	..	Water.
7·75	7·81	1744·4	1701·6	1746·	1299·6	988·	Not set.	1269·6	1030·8	Not set.	Air.
		1451·	1048·2	1186·	1090·	843·	Not set.	..	..	..	Water.

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TABLE 4.—SERIES C. SUMMARY OF TESTS. PORTLAND CEMENT BRICKS (9 × 4½ × 3)

—	Cement.				Sand.				Water.	
	Compressed.		Not Compressed.		Compressed.		Not Compressed.		Compressed.	
	Measure.	Weight.	Measure.	Weight.	Measure.	Weight.	Measure.	Weight.	Measure.	Weight.
Neat .	4·64	8·03	..	..	Nil.	Nil.	Nil.	Nil.	1·375	1·65
,,	,,	,,	..	..	,,	,,	,,	,,	,,	,,
,,	..	..	4·34	7·50	,,	,,	,,	,,	..	..
,,	..	..	,,	,,	,,	,,	,,	,,	..	..
1 to 1.	2·39	4·13	..	..	2·38	4·60	..	..	·875	1·05
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	2·42	4·19	..	..	2·31	4·45	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
2 to 1.	1·63	2·82	..	..	2·83	5·46	..	..	·75	·90
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	1·45	2·51	..	..	2·91	5·62	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
3 to 1.	1·14	1·97	..	..	3·43	6·62	..	..	·625	·75
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	1·06	1·83	..	..	3·19	6·14	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
4 to 1.	·88	1·53	..	..	3·55	6·85	..	..	·50	·60
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·82	1·42	..	..	3·29	6·36	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
5 to 1.	·72	1·25	..	..	3·64	7·02	..	..	·50	·60
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·68	1·18	..	..	3·43	6·62	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
6 to 1.	·60	1·04	..	..	3·64	7·03	..	..	·375	·45
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·58	1·00	..	..	3·48	6·71	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
7 to 1.	·52	·90	..	..	3·71	7·15	..	..	·375	·45
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·48	·84	..	..	3·44	6·64	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
8 to 1.	·46	·80	..	..	3·75	7·24	..	..	·375	·45
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·43	·74	..	..	3·44	6·63	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
9 to 1.	·42	·73	..	..	3·82	7·36	..	..	·375	·45
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·39	·66	..	..	3·48	6·72	..	..
,,	..	..	,,	,,	..	..	,,	,,	..	..
10 to 1.	·39	·66	..	..	3·84	7·41	..	..	·375	·45
,,	,,	,,	..	..	,,	,,	..	..	,,	,,
,,	..	..	·36	·60	..	..	3·52	6·79	..	..
,,	..	..	..	,,	..	..	,,	,,	..	..

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Neat, and with different proportions of Sand, tested by Crushing in a Hydraulic Press.

Water.		Average Weight of 5 Moulds, One Year Old.				Broke at Tons.				
Not Compressed.		Compressed.		Not Compressed.		Compressed.		Not Compressed.		—
Measure.	Weight.	Air.	Water.	Air.	Water.	Air.	Water.	Air.	Water.	
..	..	9·51	..	..	..	96·60	..	..	..	Neat.
..	..	..	9·76	..	..	..	132·62	..	..	,,
1·625	1·75	..	..	9·24	..	..	..	112·46	..	,,
,,	,,	..	..	..	9·47	..	..	..	113·86	,,
..	..	9·65	..	..	..	82·80	..	..	..	1 to 1
..	..	..	9·93	..	..	..	83·32	..	..	,,
1·1225	1·35	..	..	9·28	..	..	..	63·26	..	,,
,,	,,	..	..	..	9·67	..	..	..	62·74	,,
..	..	9·14	..	..	..	70·00	..	..	..	2 to 1
..	..	..	9·59	..	..	..	57·14	..	..	,,
1·00	1 20	..	..	8·85	..	..	..	41·48	..	,,
,,	,,	..	..	..	9·25	..	..	..	39·40	,,
..	..	9·13	..	..	..	53·80	..	..	..	3 to 1
..	..	..	9·56	..	..	..	26·60	..	..	,,
·9375	1·12	..	..	8·50	..	..	..	26·50	..	,,
,,	,,	..	..	..	9·32	..	..	..	21·70	,,
..	..	9·79	..	..	..	43·60	..	..	..	4 to 1
..	..	..	9·51	..	..	..	29·92	..	..	,,
·9375	1·12	..	..	8·24	..	..	..	19·82	..	,,
,,	,,	..	..	..	9·08	..	..	..	13·72	,,
..	..	8·64	..	..	..	37·40	..	..	..	5 to 1
..	..	..	9·48	..	..	..	15·10	..	..	,,
·875	1·05	..	..	8·15	..	..	..	15·28	..	,,
,,	,,	..	..	..	8·86	..	..	..	8·60	,,
..	..	8·43	..	..	..	30·28	..	..	..	6 to 1
..	..	..	9·38	..	..	..	11·24	..	..	,,
·875	1·05	..	..	7·94	..	..	..	11·00	..	,,
,,	,,	..	..	..	8·73	..	..	..	5·78	,,
..	..	8·40	..	..	..	28·88	..	..	..	7 to 1
..	..	..	9·37	..	..	..	10·54	..	..	,,
·875	1·05	..	..	7·84	..	..	..	9·05	..	,,
..	,,	..	..	..	8·70	..	..	..	4·26	,,
..	..	8·33	..	..	..	19·24	..	..	..	8 to 1
..	..	..	9·43	..	..	..	8·18	..	..	,,
·875	1·05	..	..	7·69	..	..	..	7·06	..	,,
,,	,,	..	..	..	8·56	..	..	..	2·32	,,
..	..	8·36	..	..	..	17·52	..	..	..	9 to 1
..	..	..	9·25	..	..	..	7·42	..	..	,,
.875	1·05	..	..	7·68	..	..	..	5·32	..	,,
,,	,,	..	..	..	8·54	..	..	..	2·32	,,
..	..	8·27	..	..	..	15·44	..	..	..	10 to 1
..	..	..	9·30	..	..	..	5·62	..	..	,,
·875	1·05	..	..	7·61	..	..	..	4·80	..	,,
,,	,,	..	..	..	8·75	..	..	..	2·28	,,

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C Series.—CEMENT BRICKS.

Portland Cement Bricks (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Cement, Neat.

		Weight of Five Moulds after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
		In Air.	In Water.	In Air	In Water.		
		lbs.	lbs.				
Cement	Measure 4·34 pints.	9·25	..	120·0	..	101	Not Compressed.
	Weight 7·50 lbs.	9·20	..	110·1	..	102	
Sand	Measure Nil.	9·25	..	114·6	..	103	
	Weight Nil.	9·25	..	107·0	..	104	
Water	Measure 1·625 pint.	9·26	..	110·6	..	105	
	Weight 1·75 lb.	..	9·46	..	111·4	106	
When Moulded	Oct. 1, 1867.	..	9·46	..	115·4	107	
		..	9·46	..	112·9	108	
When Tested	Oct. 1, 1868.	..	9·47	..	116·2	109	
		..	9·50	..	113·4	110	
		9·24	9·47	112·46	113·86	Average.	
Cement	Measure 4·64 pints.	9·51	..	90·0	..	101	Compressed.
	Weight 8·03 lbs.	9·50	..	96·5	..	102	
Sand	Measure Nil.	9·52	..	100·0	..	103	
	Weight Nil.	9·54	..	97·3	..	104	
Water	Measure 1·375 pint.	9·50	..	99·2	..	105	
	Weight 1·65 lb.	..	9·80	..	118·0	106	
When Moulded	Oct. 7, 1867.	..	9·75	..	143·8	107	
		..	9·73	..	128·1	108	
When Tested	Oct. 7, 1868.	..	9·78	..	146·2	109	
		..	9·75	..	127·0	110	
		9·51	9·76	96·60	132·62	Average.	

Proportion of Sand, 1 to 1.

Cement	Measure 2·42 pints.	9·27	..	65·0	..	91	Not Compressed.
	Weight 4·19 lbs.	9·28	..	64·6	..	92	
Sand	Measure 2·31 pints.	9·30	..	64·5	..	93	
	Weight 4·45 lbs.	9·32	..	62·2	..	94	
Water	Measure 1·1225 pint.	9·25	..	60·0	..	95	
	Weight 1·35 lb.	..	9·66	..	57·6	96	
When Moulded	Sept. 30, 1867.	..	9·64	..	74·0	97	
		..	9·64	..	56·8	98	
When Tested	Sept. 30, 1868.	..	9·70	..	56·9	99	
		..	9·70	..	68·4	100	
		9·28	9·67	63·26	62·74	Average.	

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C Series.—CEMENT BRICKS—*continued.*

Portland Cement Bricks (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Proportion of Sand, 1 to 1—*continued.*

			Weight of Five Moulds after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
			In Air.	In Water.	In Air.	In Water.		
			lbs.	lbs.				
Cement	Measure 2·39 pints.		9·65	..	85·0	..	91	Compressed.
	Weight 4·13 lbs.		9·67	..	84·0	..	92	
Sand	Measure 2·38 pints.		9·67	..	78·2	..	93	
	Weight 4·60 lbs.		9·65	..	85·6	..	94	
Water	Measure 0·875 pint.		9·63	..	81·2	..	95	
	Weight 1·05 lb.		..	9·91	..	88·2	96	
When Moulded	Oct. 7, 1867.		..	9·95	..	84·0	97	
			..	9·92	..	83·5	98	
When Tested	Oct. 7, 1868.		..	9·93	..	79·3	99	
			..	9·93	..	81·6	100	
			9·65	9·93	82·8	83·32	Average.	

Proportion of Sand, 2 to 1.

Cement	Measure 1·45 pint.	8·90	..	39·6	..	81	Not Compressed.
	Weight 2·51 lbs.	8·85	..	39·6	..	82	
Sand	Measure 2·91 pints.	8·85	..	42·4	..	83	
	Weight 5·62 lbs.	8·83	..	43·0	..	84	
Water	Measure 1·00 pint.	8·83	..	42·8	..	85	
	Weight 1·20 lb.	..	9·20	..	39·4	86	
When Moulded	Sept. 30, 1867.	..	9·26	..	39·4	87	
		..	9·30	..	39·8	88	
When Tested	Sept. 30, 1868.	..	9·28	..	39·2	89	
		..	9·20	..	39·2	90	
		8·85	9·25	41·48	39·40	Average.	
Cement	Measure 1·63 pint.	9·15	..	71·0	..	81	Compressed.
	Weight 2·82 lbs.	9·15	..	69·0	..	82	
Sand	Measure 2·83 pints.	9·13	..	68·2	..	83	
	Weight 5·46 lbs.	9·14	..	70·8	..	84	
Water	Measure 0·75 pint.	9·18	..	71·0	..	85	
	Weight 0·90 lb.	..	9·60	..	59·5	86	
When Moulded	Oct. 5, 1867.	..	9·61	..	56·0	87	
		..	9·57	..	55·0	88	
When Tested	Oct. 5, 1868.	..	9·58	..	60·0	89	
		..	9·58	..	55·2	90	
		9·14	9·59	70·0	57·14	Average.	

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C Series.—CEMENT BRICKS—*continued.*

Portland Cement Bricks (9" × 4½" × 3")—Neat, and with different Proportions of Sand, tested by crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Proportion of Sand, 3 to 1.

		Weight of Five Moulds after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
		In Air.	In Water.	In Air.	In Water.		
		lbs.	lbs.				
Cement	Measure 1·06 pint.	8·50	..	27·0	..	71	Not Compressed.
	Weight 1·83 lb.	8·47	..	26·0	..	72	
Sand	Measure 3·19 pints.	8·45	..	26·0	..	73	
	Weight 6·14 lbs.	8·55	..	27·0	..	74	
Water	Measure 0·9375 pint.	8·53	..	26·5	..	75	
	Weight 1·12 lb.	..	9·34	..	22·6	76	
When Moulded	Sept. 28, 1867.	..	9·31	..	18·4	77	
		..	9·30	..	22·5	78	
When Tested	Sept. 28, 1868.	..	9·36	..	22·8	79	
		..	9·32	..	22·2	80	
		8·50	9·32	26·50	21·70	Average.	
Cement	Measure 1·14 pint.	9·15	..	52·2	..	71	Compressed.
	Weight 1·97 lb.	9·13	..	54·8	..	72	
Sand	Measure 3·43 pints.	9·15	..	55·0	..	73	
	Weight 6·62 lbs.	9·12	..	53·0	..	74	
Water	Measure 0·625 pint.	9·12	..	54·0	..	75	
	Weight 0·75 lb.	..	9·58	..	28·4	76	
When Moulded	Oct. 5, 1867.	..	9·54	..	26·0	77	
		..	9·55	..	26·0	78	
When Tested	Oct. 5, 1868.	..	9·60	..	26·4	79	
		..	9·52	..	26·2	80	
		9·13	9·56	53·80	26·60	Average.	

Proportion of Sand, 4 to 1.

Cement	Measure 0·82 pint.	8·28	..	20·0	..	61	Not Compressed.
	Weight 1·42 lb.	8·20	..	19·2	..	62	
Sand	Measure 3·29 pints.	8·24	..	20·4	..	63	
	Weight 6·36 lbs.	8·20	..	20·0	..	64	
Water	Measure 0·9375 pint.	8·28	..	19·5	..	65	
	Weight 1·12 lb.	..	9·05	..	14·0	66	
When Moulded	Sept. 27, 1867.	..	9·07	..	14·2	67	
		..	9·10	..	13·5	68	
When Tested	Sept. 27, 1868.	..	9·10	..	13·5	69	
		..	9·09	..	13·4	70	
		8·24	9·08	19·82	13·72	Average.	

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C SERIES.—CEMENT BRICKS—*continued.*

PORTLAND CEMENT BRICKS (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

PROPORTION OF SAND, 4 to 1—*continued.*

		Weight of Five Moulds after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
		In Air.	In Water.	In Air.	In Water.		
		lbs.	lbs.				
Cement	Measure 0·88 pint.	8·80	..	45·0	..	61	Compressed.
	Weight 1·53 lb.	8·81	..	44·0	..	62	
Sand	Measure 3·55 pints.	8·80	..	43·0	..	63	
	Weight 6·85 lbs.	8·76	..	43·0	..	64	
Water	Measure 0·50 pint.	8·79	..	43·0	..	65	
	Weight 0·60 lb.	..	9·50	..	23·2	66	
When Moulded	Oct. 4, 1867.	..	9·56	..	20·0	67	
		..	9·50	..	21·4	68	
When Tested	Oct. 5, 1868.	..	9·50	..	20·0	69	
		..	9·50	..	20·0	70	
		8·79	9·51	43·60	20·92	Average.	

PROPORTION OF SAND, 5 to 1.

		Weight of each Brick, being the average of Five after being kept One Year.		Broke at Tons.		Number of Tests.	Remarks.
		In Air.	In Water.	In Air.	In Water.		
		lbs.	lbs.				
Cement	Measure 0·68 pint.	8·12	..	15·8	..	51	Not Compressed.
	Weight 1·18 lb.	8·17	..	14·6	..	52	
Sand	Measure 3·43 pints.	8·19	..	15·8	..	53	
	Weight 6·62 lbs.	8·15	..	15·2	..	54	
Water	Measure 0·875 pint.	8·15	..	15·0	..	55	
	Weight 1·05 lb.	..	8·86	..	8·7	56	
When Moulded	Sept. 26, 1867.	..	8·92	..	8·5	57	
		..	8·84	..	9·0	58	
When Tested	Sept. 26, 1868.	..	8·86	..	8·5	59	
		..	8·83	..	8·3	60	
		8·15	8·86	15·28	8·60	Average.	

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C Series.—CEMENT BRICKS—*continued.*

Portland Cement Bricks (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Proportion of Sand 5 to 1—*continued.*

		Weight of each Brick, being the average of Five after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
		In Air.	In Water.	In Air.	In Water.		
		lbs.	lbs.				
Cement	Measure 0·72 pint.	8·64	..	37·0	..	51	Compressed.
	Weight 1·25 lb.	8·64	..	37·0	..	52	
Sand	Measure 3·64 pints.	8·65	..	37·5	..	53	
	Weight 7·02 lbs.	8·63	..	38·5	..	54	
Water	Measure 0·50 pint.	8·63	..	37·0	..	55	
	Weight 0·60 lb.	..	9·50	..	15·0	56	
When Moulded	Oct. 4, 1867.	..	9·50	..	15·8	57	
		..	9·54	..	15·2	58	
When Tested	Oct. 5, 1868.	..	9·47	..	15·0	59	
		..	9·40	..	14·5	60	
		8·64	9·48	37·40	15·10	Average.	

Proportion of Sand, 6 to 1.

Cement	Measure 0·58 pint.	7·98	..	10·5	..	41	Not Compressed.
	Weight 1·00 lb.	7·95	..	11·0	..	42	
Sand	Measure 3·48 pints.	7·90	..	11·0	..	43	
	Weight 6·71 lbs.	7·98	..	11·0	..	44	
Water	Measure 0·875 pint.	7·90	..	11·5	..	45	
	Weight 1·05 lb.	..	8·80	..	6·4	46	
When Moulded	Sept. 26, 1867.	..	8·85	..	5·9	47	
		..	8·67	..	5·9	48	
When Tested	Sept. 26, 1868.	..	8·65	..	5·5	49	
		..	8·68	..	5·2	50	
		7·94	8·73	11·00	5·78	Average.	
Cement	Measure 0·60 pint.	8·40	..	30·6	..	41	Compressed.
	Weight 1·04 lb.	8·41	..	30·4	..	42	
Sand	Measure 3·64 pints.	8·44	..	30·4	..	43	
	Weight 7·03 lbs.	8·44	..	30·0	..	44	
Water	Measure 0·375 pint.	8·45	..	30·0	..	45	
	Weight 0·45 lb.	..	9·37	..	10·8	46	
When Moulded	Oct. 3, 1867.	..	9·38	..	11·4	47	
		..	9·42	..	12·0	48	
When Tested	Oct. 3, 1868.	..	9·38	..	11·0	49	
		..	9·37	..	11·0	50	
		8·43	9·38	30·28	11·24	Average.	

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C Series.—CEMENT BRICKS—*continued.*

Portland Cement Bricks (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Proportion of Sand, 7 to 1.

		Weight of each Brick, being the average of Five after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
		In Air.	In Water.	In Air.	In Water.		
		lbs.	lbs.				
Cement	Measure 0·48 pint.	7·74	..	8·9	..	31	Not Compressed.
	Weight 0·84 lb.	7·84	..	8·4	..	32	
Sand	Measure 3·44 pints.	7·82	..	9·0	..	33	
	Weight 6·64 lbs.	7·90	..	9·0	..	34	
Water	Measure 0·875 pint.	7·90	..	9·8	..	35	
	Weight 1·05 lb.	..	8·69	..	4·0	36	
When Moulded	Sept. 25, 1867.	..	8·67	..	3·2	37	
		..	8·67	..	4·2	38	
When Tested	Sept. 26, 1868.	..	8·71	..	4·8	39	
		..	8·77	..	5·1	40	
		7·84	8·705	9·05	4·26	Average.	
Cement	Measure 0·52 pint.	8·40	..	28·6	..	31	Compressed.
	Weight 0·90 lb.	8·38	..	28·2	..	32	
Sand	Measure 3·71 pints.	8·40	..	28·8	..	33	
	Weight 7·15 lbs.	8·42	..	28·8	..	34	
Water	Measure 0·375 pint.	8·41	..	30·0	..	35	
	Weight 0·45 lb.	..	9·37	..	10·5	36	
When Moulded	Oct. 3, 1867.	..	9·35	..	11·2	37	
		..	9·42	..	10·4	38	
When Tested	Oct. 3, 1868.	..	9·39	..	10·4	39	
		..	9·35	..	10·2	40	
		8·40	9·37	28·88	10·54	Average.	

Proportion of Sand, 8 to 1.

Cement	Measure 0·43 pint.	7·70	..	8·0	..	21	Not Compressed.
	Weight 0·74 lb.	7·70	..	7·0	..	22	
Sand	Measure 3·44 pints.	7·75	..	6·7	..	23	
	Weight 6·63 lbs.	7·62	..	6·9	..	24	
Water	Measure 0·875 pint.	7·70	..	6·7	..	25	
	Weight 1·05 lb.	..	8·60	..	2·3	26	
When Moulded	Sept. 24, 1867.	..	8·55	..	2·2	27	
		..	8·56	..	2·6	28	
When Tested	Sept. 25, 1868.	..	8·55	..	2·2	29	
		..	8·55	..	2·3	30	
		7·694	8·562	7·06	2·32	Average.	

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C Series.—CEMENT BRICKS—*continued.*

Portland Cement Bricks (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Proportion of Sand, 8 to 1—*continued.*

		Weight of each Brick, being the average of Five after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
		In Air.	In Water.	In Air.	In Water.		
		lbs.	lbs.				
Cement	Measure 0·46 pint.	8·33	..	19·4	..	21	Compressed.
	Weight 0·80 lb.	8·34	..	19·0	..	22	
Sand	Measure 3·75 pints.	8·35	..	18·8	..	23	
	Weight 7·24 lbs.	8·32	..	19·0	..	24	
Water	Measure 0·375 pint.	8·32	..	19·0	..	25	
	Weight 0·45 lb.	..	9·42	..	8·2	26	
When Moulded	Oct. 2, 1867.	..	9·50	..	8·1	27	
		..	9·43	..	8·2	28	
When Tested	Oct. 2, 1868.	..	9·40	..	8·4	29	
		..	9·40	..	8·0	30	
		8·33	9·43	19·24	8·18	Average.	

Proportion of Sand, 9 to 1.

Cement	Measure 0·39 pint.	7·70	..	5·4	..	11	Not Compressed.
	Weight 0·66 lb.	7·70	..	5·2	..	12	
Sand	Measure 3·48 pints.	7·68	..	5·2	..	13	
	Weight 6·72 lbs.	7·70	..	5·5	..	14	
Water	Measure 0·875 pint.	7·65	..	5·3	..	15	
	Weight 1·05 lb.	..	8·55	..	2·2	16	
When Moulded	Sept. 24, 1867.	..	8·54	..	2·2	17	
		..	8·54	..	2·2	18	
When Tested	Sept. 24, 1868.	..	8·56	..	2·5	19	
		..	8·54	..	2·5	20	
		7·686	8·546	5·32	2·32	Average.	
Cement	Measure 0·42 pint.	8·38	..	17·8	..	11	Compressed.
	Weight 0·73 lb.	8·32	..	17·6	..	12	
Sand	Measure 3·82 pints.	8·40	..	17·8	..	13	
	Weight 7·36 lbs.	8·30	..	17·0	..	14	
Water	Measure 0·375 pint.	8·39	..	17·4	..	15	
	Weight 0·45 lb.	..	9·24	..	7·5	16	
When Moulded	Oct. 2, 1867.	..	9·26	..	7·4	17	
		..	9·26	..	7·5	18	
When Tested	Oct. 2, 1868.	..	9·23	..	7·2	19	
		..	9·27	..	7·5	20	
		8·36	9·25	17·52	7·42	Average.	

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C Series.—CEMENT BRICKS—*continued.*

Portland Cement Bricks (9″ × 4½″ × 3″)—Neat, and with different Proportions of Sand, tested by Crushing in Hydraulic Press.

Weight of one Bushel of Cement 110·56 lbs.

Proportion of Sand, 10 to 1.

				Weight of Five Moulds after being kept One Year.		Broke at Tons.		Number of Test.	Remarks.
				In Air.	In Water.	In Air.	In Water.		
				lbs.	lbs.				
Cement	Measure	0·36	pint.	7·64	..	4·50	..	1	Moulds filled in the ordinary way, without Compression.
	Weight	0·60	lb.	7·60	..	4·60	..	2	
Sand	Measure	3·52	pints.	7·56	..	4·50	..	3	
	Weight	6·79	lbs.	7·64	..	5·40	..	4	
Water	Measure	0·875	pint.	7·64	..	5·00	..	5	
	Weight	1·05	lb.	..	8·80	..	2·50	6	
When Moulded	Sept. 23, 1867.			..	8·82	..	2·50	7	
When Tested	Sept. 23, 1868.			..	8·70	..	2·20	8	
				..	8·65	..	2·10	9	
				..	8·80	..	2·10	10	
				7·616	8·754	4·80	2·28	Average.	
Cement	Measure	0·39	pint.	8·23	.	14·6	..	..	Moulds filled and Compressed by ramming.
	Weight	0·66	lb.	8·27	..	16·6	..	..	
Sand	Measure	3·84	pints.	8·30	..	16·6	..	..	
	Weight	7·41	lbs.	8·27	..	15·0	..	..	
Water	Measure	0·375	pint.	8·30	..	15·0	..	..	
	Weight	0·45	lb.	..	9·30	..	5·5	..	
When Moulded	Oct. 1, 1867.			..	9·25	..	5·0	..	
When Tested	Oct. 1, 1868.			..	9·31	..	5·5	..	
				..	9·32	..	5·7	..	
				..	9·33	..	6·4	..	
				8·27	9·30	15·44	5·62	Average.	

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D Series.—CONCRETE BLOCKS.

PORTLAND CEMENT CONCRETE BLOCKS of various Materials, set and kept in Air for One Year, also set and *kept in Water* for the same time.

TABLE I.

Material—Ballast.
Size of Block—12″ × 12″ × 12″. Compressed.

Proportion.	Weight in lbs.			Weight of each Block in lbs.		Crushed at Tons.		Remarks.
	Cement.	Sand.	Water.	Kept in Air.	Kept in Water.	Air.	Water.	
1 to 1	59·36	66·96	16·00	137·60	147·25	107·0*	170·50	* Exceptional.
2 „ 1	42·64	96·40	12·00	142·60	152·50	149·0	160·0	
3 „ 1	32·00	108·56	10·00	145·25	152·25	113·0	115·50	
4 „ 1	25·84	116·96	8·80	145·75	152·50	103·0	108·50	
5 „ 1	21·28	120·24	8·00	142·10	150·95	89·0	99·50	
6 „ 1	18·08	122·48	8·00	141·56	150·00	80·50	91·0	
7 „ 1	15·84	125·04	7·60	141·70	150·20	75·0	80·50	
8 „ 1	14·08	127·04	7·60	142·30	150·80	61·50	76·0	
9 „ 1	12·64	128·64	7·20	142·10	151·50	54·0	68·50	
10 „ 1	11·36	128·88	6·80	142·00	150·00	48·50	48·0	

TABLE II.

Material—Ballast.
Size of Block—6″ × 6″ × 6″. Compressed.

1 to 1	7·42	8·37	2·00	17·50	18·04	38·0	33·60
2 „ 1	5·33	12·05	1·50	17·78	18·97	43·0	34·50
3 „ 1	4·00	13·57	1·25	18·28	19·35	30·0	35·50
4 „ 1	3·23	14·62	1·10	18·28	18·71	30·0	28·00
5 „ 1	2·66	15·03	1·00	18·26	18·98	24·50	35·50
6 „ 1	2·26	15·31	1·00	17·90	18·60	20·40	19·60
7 „ 1	1·98	15·63	·95	17·85	18·85	16·50	16·0
8 „ 1	1·76	15·88	·95	17·86	18·90	13·50	13·50
9 „ 1	1·58	16·08	·90	17·78	19·0	12·0	11·00
10 „ 1	1·42	16·11	·85	17·68	18·70	10·50	10·50

TABLE III.

Material—Ballast.
Size of Block—6″ × 6″ × 6″. Not compressed.

1 to 1	7·12	8·04	1·92	16·44	17·60	30·0	37·50
2 „ 1	4·90	11·09	1·38	17·57	18·03	38·50	36·00
3 „ 1	3·56	12·11	1·11	17·75	18·98	24·0	28·00
4 „ 1	2·85	12·92	·97	17·84	18·28	28·0	27·00
5 „ 1	2·33	13·18	·87	17·90	18·73	24·0	23·50
6 „ 1	2·00	13·49	·88	17·35	18·30	18·20	17·00
7 „ 1	1·77	14·02	·85	17·32	17·90	14·0	12·50
8 „ 1	1·60	14·51	·85	17·38	17·95	12·50	11·00
9 „ 1	1·43	14·59	·80	17·40	17·97	10·0	9·00
10 „ 1	1·26	14·35	·75	17·20	17·50	8·0	7·00

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D Series.—CONCRETE BLOCKS—*continued.*

Portland Cement Concrete Blocks of various Materials, set and kept in Air for One Year, also set and *kept in Water* for the same time.

Table IV.

Materials—Various.
Size of Block—12″ × 12″ × 12″. Compressed.

Proportion.	Weight in lbs.			Weight of each Block in lbs.		Crushed at Tons.		Remarks.
	Cement.	Sand.	Water.	Kept in Air.	Kept in Water.	Air.	Water.	
6 to 1	18·08	122·48	8·00	141·56	150·0	80·50	91·0	Ballast.
	19·20	102·40	11·60	127·80	140·50	118·0	138·50	Portland stone.
	18·48	116·48	10·40	143·50	147·54	113·20	96·50	Granite.
	17·28	106·64	14·00	128·95	139·45	109·20	136·50	Pottery.
	16·88	98·32	12·80	122·50	131·54	110·50	111·0	Slag.
	16·24	111·28	11·60	131·90	141·33	116·00	126·0	Flints.
	18·96	124·08	10·40	147·70	155·50	99·00	112·50	Glass.

Table V.

Materials—Various.
Size of Block—12″ × 12″ × 12″. Compressed.

8 to 1	14·08	127·04	7·60	142·30	150·80	61·50	76·0	Ballast.
	14·88	106·08	13·20	130·00	140·30	110·0	126·50	Portland stone.
	12·24	116·56	10·00	140·20	144·68	73·80	84·60	Granite.
	13·12	108·24	12·80	127·40	136·20	97·50	118·0	Pottery.
	12·80	98·96	12·00	114·60	127·85	85·20	70·0	Slag.
	12·48	114·24	10·40	133·10	140·12	103·50	117·50	Flints.
	14·56	126·32	9·20	143·20	153·23	65·0	94·0	Glass.

Table VI.

Materials—Various.
Size of Block—12″ × 12″ × 12″. Compressed.

10 to 1	11·36	128·88	6·80	142·0	150·0	48·50	48·0	Ballast.
	12·16	108·48	14·88	128·0	143·10	72·60	78·0	Portland stone.
	9·84	116·80	9·20	139·85	142·80	49·80	60·50	Granite.
	10·72	110·00	11·60	126·80	136·00	90·0	100·0	Pottery.
	10·24	99·68	11·20	113·25	124·80	60·0	52·0	Slag.
	10·08	114·96	10·20	130·00	141·00	70·0	98·0	Flints.
	11·84	129·28	8·40	143·30	152·10	53·0	75·0	Glass.

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D Series.—CONCRETE BLOCKS—*continued.*

Portland Cement Concrete Blocks of various Materials, set and kept in Air for One Year, also set and *kept in Water* for the same time.

Table VII.

Materials—Various.
Size of Block—6″ × 6″ × 6″. Compressed.
When Moulded, Nov. 4, 1867. When Tested, Nov. 4, 1868.

Proportion.	Weight in lbs.			Weight of each Block in lbs.		Crushed at Tons.		Remarks.
	Cement.	Sand.	Water.	Kept in Air.	Kept in Water.	Air.	Water.	
6 to 1	2·26	15·31	1·00	17·90	18·60	20·40	19·60	Ballast.
	2·40	12·80	1·45	16·90	17·67	40·60	34·50	Portland stone
	2·31	14·56	1·30	18·53	18·88	30·50	27·0	Granite.
	2·16	13·33	1·75	16·10	17·50	28·80	26·50	Pottery.
	2·11	12·29	1·60	15·08	16·61	23·0	23·50	Slag.
	2·03	13·91	1·45	16·50	17·65	20·50	24·00	Flints.
	2·37	15·51	1·30	18·50	19·25	28·0	23·00	Glass.

Table VIII.

Materials—Various.
Size of Block—6″ × 6″ × 6″. Compressed.
When Moulded, Nov. 6, 1867. When Tested, Nov. 6, 1868.

8 to 1	1·76	15·88	·95	17·86	18·90	13·50	13·50	Ballast.
	1·86	13·26	1·65	16·32	17·50	33·00	29·00	Portland stone.
	1·53	14·57	1·25	18·10	19·00	19·60	16·00	Granite.
	1·64	13·53	1·60	16·15	16·95	22·00	23·00	Pottery.
	1·60	12·37	1·50	14·20	15·91	19·50	13·40	Slag.
	1·56	14·28	1·30	16·45	17·62	17·50	20·00	Flints.
	1·82	15·79	1·15	18·00	18·93	18·00	17·50	Glass.

Table IX.

Materials—Various.
Size of Block—6″ × 6″ × 6″. Compressed.
When Moulded, Nov. 8, 1867. When Tested, Nov. 9, 1868.

10 to 1	1·42	16·11	·85	17·68	18·70	10·50	10·50	Ballast.
	1·52	13·56	1·86	16·44	17·90	22·0	16·50	Portland stone.
	1·23	14·60	1·15	17·60	18·50	15·50	12·40	Granite.
	1·34	13·75	1·45	16·09	16·80	18·50	19·00	Pottery.
	1·28	12·46	1·40	14·00	15·50	10·50	10·00	Slag.
	1·26	14·37	1·15	16·15	17·60	15·00	18·50	Flints.
	1·48	16·16	1·05	17·90	18·73	12·50	12·80	Glass.

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D Series.—CONCRETE BLOCKS—*continued.*

PORTLAND CEMENT CONCRETE BLOCKS of various Materials, set and kept in Air for One Year, also set and *kept in Water* for the same time.

TABLE X.

Materials—Various.
Size of Block—6″ × 6″ × 6″. Not Compressed.

Proportion.	Weight in lbs.			Weight of each Block in lbs.		Crushed at Tons.		Remarks.
	Cement.	Sand.	Water.	Kept in Air.	Kept in Water.	Air.	Water.	
6 to 1	2·00	13·49	·88	17·35	18·30	18·20	17·00	Ballast.
	2·23	11·91	1·35	15·70	17·12	30·00	23·0	Portland stone.
	1·98	12·55	1·12	17·40	18·30	24·50	15·50	Granite.
	1·91	11·78	1·54	15·85	17·00	24·60	24·00	Pottery.
	1·95	11·36	1·23	14·80	15·77	20·00	19·20	Slag.
	1·76	12·11	1·26	15·20	16·60	15·50	15·50	Flints.
	2·04	13·35	1·11	17·76	18·40	16·50	17·00	Glass.

TABLE XI.

Materials—Various.
Size of Block—6″ × 6″ × 6″. Not Compressed.

8 to 1	1·60	14·51	·85	17·38	17·95	12·50	11·00	Ballast.
	1·70	12·15	1·51	15·63	17·00	24·50	19·50	Portland stone.
	1·31	12·53	1·07	17·45	18·20	14·50	13·40	Granite.
	1·43	11·83	1·40	15·71	16·70	18·00	18·00	Pottery.
	1·47	11·37	1·37	13·73	15·12	14·00	9·50	Slag.
	1·34	12·02	1·12	15·40	16·60	14·00	12·50	Flints.
	1·52	13·22	·96	17·30	17·80	13·60	11·00	Glass.

TABLE XII.

Materials—Various.
Size of Block—6″ × 6″ × 6″.—Not Compressed.

10 to 1	1·26	14·35	·75	17·20	17·50	8·00	7·00	Ballast.
	1·38	12·34	1·69	16·15	17·00	19·00	10·00	Portland stone.
	1·06	12·62	·99	16·97	17·63	11·50	11·00	Granite.
	1·15	11·88	1·25	15·66	16·50	14·00	17·00	Pottery.
	1·17	11·43	1·28	13·75	15·00	8·50	5·50	Slag.
	1·13	12·00	·90	15·12	16·60	12·80	11·00	Flints.
	1·22	13·31	·86	16·95	17·70	10·00	10·80	Glass.

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D Series.—CONCRETE BLOCKS—*continued.*

Portland Cement Concrete Blocks of various Materials, set and kept in Air for One Year, also set and *kept in Water* for the same time.

Table XIII.

Materials—Various.
Size of Block—6″ × 6″ × 6″.

Proportion.	Weight of Cement. lbs.		Weight of Sand, &c. lbs.		Weight of Water. lbs.		Weight of each Block. lbs.		Crushed at Tons.		Remarks.
	Compressed.	Not Compressed.	Compressed.	Not Compressed.	Compressed.	Not Compressed.	Compressed.	Not Compressed.	Compressed.	Not Compressed.	
											Kept in Air.
6 to 1	2·26	2·00	15·31	13·49	1·00	·88	17·90	17·35	20·40	18·20	Ballast.
	2·40	2·23	12·80	11·91	1·45	1·35	16·90	15·70	40·60	30·00	Portland stone.
	2:31	1·98	14·56	12·55	1·30	1·12	18·58	17·40	30·50	24·50	Granite.
	2·16	1·91	13·33	11·78	1·75	1·54	16·10	15·85	28·80	24·60	Pottery.
	2·11	1·95	12·29	11·36	1·60	1·23	15·08	14·80	23·00	20·00	Slag.
	2·03	1·76	13·91	12·11	1·45	1·26	16·50	15·20	20·50	15·50	Flints.
	2·37	2·04	15·51	13·35	1·30	1·11	18·50	17·76	28·00	16·50	Glass.

Table XIV.

Materials—Various.
Size of Block—6″ × 6″ × 6″.

											Kept in Water.
6 to 1	2·26	2·00	15·31	13·4	91·00	·88	18·60	18·30	19·60	17·00	Ballast.
	2·40	2·23	12·80	11·9	11·45	1·35	17·67	17·12	34·50	23·00	Portland stone.
	2·31	1·98	14·56	12·5	51·30	1·12	18·88	18·30	27·00	15·15	Granite.
	2·16	1·91	13·33	11·7	81·75	1·54	17·50	17·00	26·50	24·00	Pottery.
	2·11	1·95	12·29	11·3	61·60	1·23	16·61	15·77	23·50	19·20	Slag.
	2·03	1·76	13·91	12·1	11·45	1·26	17·65	16·60	24·00	15·50	Flints.
	2·37	2·04	15·51	13·3	51·30	1·11	19·25	18·40	23·00	17·00	Glass.

Table XV.

Materials—Various.
Size of Block—6″ × 6″ × 6″.

											Kept in Air.
8 to 1	1·76	1·60	15·88	14·51	·95	·85	17·86	17·38	13·50	12·50	Ballast.
	1·86	1·70	13·26	12·15	1·65	1·51	16·32	15·63	33·00	24·50	Portland stone.
	1·53	1·31	14·57	12·53	1·25	1·07	18·10	17·45	19·60	14·50	Granite.
	1·64	1·43	13·53	11·83	1·60	1·40	16·15	15·71	22·00	18·00	Pottery.
	1·60	1·47	12·37	11·87	1·50	1·37	14·20	13·73	19·50	14·00	Slag.
	1·56	1·34	14·28	12·02	1·30	1·12	16·45	15·40	17·50	14·00	Flints.
	1·82	1·52	15·79	13·22	1·15	·96	18·00	17·30	18·00	13·60	Glass.

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D Series.—CONCRETE BLOCKS—*continued.*

Portland Cement Concrete Blocks of various Materials, set and kept in Air for One Year, also set and *kept in Water* for the same time.

Table XVI.

Materials—Various.
Size of Block—6″ × 6″ × 6″.

Proportion.	Weight of Cement. lbs.		Weight of Sand, &c. lbs.		Weight of Water. lbs.		Weight of each Block. lbs.		Crushed at Tons.		Remarks.
	Compressed.	Not Compressed.	Compressed.	Not Compressed.	Compressed.	Not Compressed.	Compressed.	Not Compressed.	Compressed.	Not Compressed.	
											Kept in Water.
8 to 1	1·76	1·60	15·88	14·51	·95	·85	18·90	17·95	13·50	11·00	Ballast.
	1·86	1·70	13·26	12·15	1·65	1·51	17·50	17·00	24·50	19·50	Portland stone.
	1·53	1·31	14·57	12·53	1·25	1·07	19·00	18·20	16·00	13·40	Granite.
	1·64	1·43	13·53	11·83	1·60	1·40	16·95	16·70	23·00	18·00	Pottery.
	1·60	1·47	12·37	11·37	1·50	1·37	15·91	15·12	13·40	9·50	Slag.
	1·56	1·34	14·28	12·02	1·30	1·12	17·62	16·60	20·00	12·50	Flints.
	1·82	1·52	15·79	13·22	1·15	·96	18·93	17·80	17·50	11·00	Glass.

Table XVII.

Materials—Various.
Size of Block—6″ × 6″ × 6″.

											Kept in Air.
10 to 1	1·42	1·26	16·11	14·35	·85	·75	17·68	17·20	10·50	8·00	Ballast.
	1·52	1·38	13·56	12·34	1·86	1·69	16·44	16·15	22·00	19·00	Portland stone.
	1·23	1·06	14·60	12·62	1·15	·99	17·40	16·97	15·50	11·50	Granite.
	1·34	1·15	13·75	11·88	1·45	1·25	16·09	15·66	18·50	14·00	Pottery.
	1·28	1·17	12·46	11·43	1·40	1·28	14·00	13·75	10·50	8·50	Slag.
	1·26	1·13	14·37	12·00	1·15	·90	16·15	15·12	15·00	12·80	Flints.
	1·48	1·22	16·16	13·31	1·05	·86	17·90	16·95	12·50	10·00	Glass.

Table XVIII.

Materials—Various.
Size of Block—6″ × 6″ × 6″.

											Kept in Water.
10 to 1	1·42	1·26	16·11	14·35	·85	·75	18·70	17·50	10·50	7·00	Ballast.
	1·52	1·38	13·56	12·34	1·86	1·69	17·90	17·00	16·50	10·00	Portland stone.
	1·23	1·06	14·60	12·62	1·15	·99	18·50	17·63	12·40	11·00	Granite.
	1·34	1·15	13·75	11·88	1·45	1·25	16·80	16·50	19·00	17·00	Pottery.
	1·28	1·17	12·46	11·43	1.40	1·28	15·50	15·00	10·00	5·50	Slag.
	1·26	1·13	14·37	12·00	1·15	·90	17·60	16·60	18·50	11·00	Flints.
	1·48	1·22	16·16	13·31	1·05	·86	18·73	17·70	12·80	10·80	Glass.

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TABLE 5.

CONCRETE SEWERS.

LOCALITY.	Size (internally).	Length in Feet.	Cost per Foot Lineal, irrespective of Earthworks and Adjuncts.	REMARKS.
	ft. in. ft. in.		s. d.	
Tooting . . .	3 9 × 2 6	775	11 4	P. C. concrete 6 to 1, and rendered with P. C. cement 1 to 1.
Bridge-road, Battersea . . .	4 0 × 2 8	644	13 5	Ditto.
Camberwell-road.	4 0 × 2 8	2,355	10 0	Ditto, but 7 to 1.
Falcon Brook .	4 3 × 2 10	2,030	14 10	Ditto, 6 to 1.
Earl Sewer, Deptford Lower road	7 1 × 7 1	1,023	16 0	Only lower half rendered as above.
CONCRETE AND HALF-BRICK SEWERS.				
Blackfriars-road .	4 6 × 3 0	3,486	11 9	Half-brick in cement, and P. C. concrete 7 to 1.
Newington Causeway	4 6 × 3 0	1,953	12 2½	Ditto.

TABLE 6.

SEWER AT DULWICH.

REGISTER of Experiments made with Portland Cement. Form of Mould, No. 2, Table 1. Sectional Area, 2·25 Square Inches. Manufactured by Messrs. FORMBY. October 17, 1868—January 28, 1869.

Weight per Bushel.	When Moulded.	When Tested.	Days in Mould.	Averages of 5 Tests each.		Proportionate Strength of the 1 to 1 to Neat.
				Broke at lbs.	Broke at lbs.	
				Neat.	1 to 1.	
lbs. 118	Oct. 17	Oct. 24	7	805·2	275·8	34·25
,,	,,	Oct. 31	14	924·4	316·0	34·18
,,	Oct. 19	Nov. 9	21	982·6	388·2	39·50
,,	,,	Nov. 19	Months. 1	995·4	384·4	38·61
,,	Oct. 28	Dec. 28	2	1237·0	479·0	38·71
,,	,,	Jan. 28	3	1239·0	538·6	43·47

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TABLE 7.

CONTRACTS ON THE SOUTH SIDE.

PARTICULARS of Portland Cement supplied during the last 5 years, with average breaking weight at seven days. Form of Mould, No. 2, Table 1. Sectional Area, 2·25 Square Inches.

Names of Manufacturers and Agents.	Quantity in Bushels.	Average Weight per Bushel.	Number of Tests.	Average Breaking Weight.
		lbs.		lbs.
Formby	31,581	118·27	550	862·01
Booth	12,464	119·75	80	846·50
Lee and Co.	512	120·00	10	839·00
Burham Brick and Cement Company	320,716	113·54	3,705	825·73
Casson and Co., Agents	5,200	114·50	50	816·80
Knight, Bevan, and Sturge	19,429	114·52	820	803·38
Robins and Co. (Limited)	68,880	118·00	620	795·31
White and Co.	60	119·00	10	791·70
Burge and Co., Agents.	4,500	113·00	30	789·30
Hilton	103,453	117·17	1,300	786·99
Beaumont, Agent	40	116·00	10	765·00
Lavers, Agent	12,002	116·17	160	706·97
Weston	600	120·00	10	666·40
Young and Son, Agents	200	117·00	10	655·80
Coles and Shadbolt	240	107·00	10	580·00
Tingey	6,300	115·50	100	564·27
Harwood and Hatcher, Agents	3,040	117·78	30	408·03
Generally	589,217	115·23	7,505	806·63

TABLE 8.

SPECIFIED Standard of Portland Cement supplied by Manufacturers.

	Breaking Weights.	
	At per Square Inch.	On 2¼ Square Inches.
	lbs.	lbs.
Greenwich and Deptford sewers	222·2	500
Woolwich sewers	250	562·5
White Post-lane	,,	,,
The Earl, Battle Bridge, &c.	,,	,,
Falcon Brook	,,	,,
Heathwall	,,	,,
Southern Thames Embankment	,,	,,
Southwark Park	350	787·5
Bankside	,,	,,
Bermondsey-street, &c.	,,	,,
Belvedere-road, &c.	,,	,,
Plough-road	,,	,,
Putney Boundary	,,	,,
Kennington Park-road	,,	,,
Kennington and Lambeth	,,	,,

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TABLE 9.

SIXTY Experiments on Concrete Briquettes subjected to a tensile strain. Cement weighing 112 lbs. per Bushel. Form of Mould, No. 2, Table 1. Sectional Area, 2·25 Square Inches. Average of Two in each case.

1 Month.	6 Weeks.	2 Months.	6 Months.	12 Months.	——
lbs.	lbs.	lbs.	lbs.	lbs.	
306·0	383·0	407·5	505·5	541·0	3 to 1
403·5	397·5	411·0	479·0	554·5	4 to 1
Broke winding up.	246·0	269·5	439·0	482·0	5 to 1
133·5	189·5	221·0	273·0	319·0	6 to 1
159·0	186·0	215·0	280·5	368·0	7 to 1
103·0	143·0	140·5	282·5	352·5	8 to 1

EXTRACTS from Specification for Sewers made of Portland Cement Concrete, and Concrete lined with Brickwork.

"7. 1,953 feet run of egg-shaped sewer, 4 feet 6 inches in height and 3 feet in width in the clear, half brick in thickness in Portland cement, to be surrounded with Portland cement concrete, with a 9-inch stoneware pipe under invert of sewer, as shown on Drawing No. 6 (Plate 15, Fig. 7), commencing by an existing bell-mouth at Stone's End, from thence along Newington Causeway to an existing eye opposite London Road. (See Clause 34.)

"8. 3,486 feet run of egg-shaped sewer, 4 feet 6 inches in height by 3 feet in width in the clear, half brick in thickness, to be surrounded with Portland cement concrete, and a 9-inch stoneware pipe under invert of sewer, as shown on Drawing No. 7, commencing at an existing bell-mouth in Saint George's Circus, thence along Blackfriars Road, to near Blackfriars Bridge. (See Clause 34.)

"9. 2,355 feet run of egg-shaped sewer, 4 feet in height and 2 feet 8 inches in width in the clear, to be in Portland cement concrete, with a 9-inch stoneware pipe under invert of sewer, as shown on Drawing No. 8 (Plate 15, Fig. 9), commencing near Wyndham Road, from thence along Camberwell Road and High Street to Cold Harbour Lane, there to be connected with the existing sewer by a vertical shaft.

"10. 1,023 feet run of barrel sewer, 7 feet 1 inch inside diameter, to be in Portland cement concrete, and constructed, as shown on Drawing No. 9 (Plate 15, Fig. 10), along the line of Open Earl Sewer between Grand Surrey Canal and Deptford Lower Road. The ballast for the concrete sewers is to be clean, fine, sharp Thames ballast, or other of equal quality, to be free from clay or other earthy substances, and mixed and used in every respect as the Engineer may direct. (See Clause 33.)

CONCRETE SEWERS.

"33. The sewers in Camberwell Road and the Earl, shown on Drawings 8 and 9 (Plate 15, Figs 9 and 10), are to be *entirely* constructed of Portland cement concrete, made in the proportion of three bushels of cement to one cubic yard of sand and ballast. The same kind of concrete is to be used for the entire cross section,

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including that which surrounds the pipe under invert. In making this concrete, a box 3 feet, by 3 feet by 3 feet, is to be used for the ballast, and another box capable of holding three 'striked' bushels for the cement. The cement and ballast are to be turned over at least three times, and thoroughly mixed together.

"33a. The centering used for the arches is to be covered with thin sheet iron, copper, or zinc, and, if necessary, greased to insure a smooth face for the arch. The invert below the springing line is to be rendered with Portland cement and sand, in equal proportions, and finished off half an inch thick with a smooth trowelled face. This work is to be done by a plasterer. The concrete for the arches is to be of sand and cement next the face, and wherever the face is rough, it is to be touched up by a plasterer, after the centres are drawn; and left in a neat and workmanlike manner. The concrete side entrances are to be formed of cement and ballast of the same proportions and quality as that for the concrete sewers, and are to be *entirely* rendered with cement half an inch thick and finished like the sewers.

Half-brick Sewers.

"34. The sewers in Newington Causeway and Blackfriars Road, Drawings 6 and 7 (Plate 15, Fig. 7), are to be of half brick in thickness, and to be surrounded by Portland cement concrete of the same proportion, and in every respect like that described in the Clause, No. 33. Below the springing line, that part of the sewer which is of concrete is to be formed first, and is to be allowed to set hard before the brickwork is commenced. The brickwork is to be entirely of the best picked and assorted No. 1 bricks, and to have a collar joint between it and the concrete of half an inch of cement.

Cement.

"37. The whole of the cement shall be Portland cement of the very best quality, ground extremely fine, weighing not less than 112 lbs. to the striked bushel, and capable of maintaining a breaking weight of 350 lbs. per square inch seven days after being made in a mould, and immersed in water during the interval of seven days. The Contractor shall at all times keep in store upon the works a supply of cement equal to at least fourteen days' requirements; and with each delivery of cement, shall send to the Clerk of Works a memorandum of the number of bushels sent in, and the name of the manufacturer.

"38. The cement as hereinbefore described shall, except where otherwise specified, be mixed in the proportion of one of Portland cement to one of sand. No cement to be used which has become hard or set.

Concrete Sewer with Brick lining.

"The concrete for the 9 feet 6 inches by 9 feet 6 inches sewer shown on Drawings Nos. 1 and 3 (Plate 15, Fig. 11), is to be Portland cement concrete, with a lining of brickwork 4½ inches thick in Portland cement, the concrete to be made in the proportion of three bushels of cement to one cubic yard of sand and ballast. In making this concrete, a box 3 feet, by 3 feet by 3 feet, is to be used for the ballast, and another box capable of holding three striked bushels for the cement. The cement and ballast are to be turned over at least three times, and thoroughly mixed together. Below the springing line that part of the sewer which is of concrete is to be formed first, and is to be allowed to set hard before the brickwork is commenced; the brickwork is to be entirely

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of the best picked and assorted No. 1 bricks, and to have a collar joint of cement half an inch in thickness between it and the concrete."

CONCRETE.

"40. Excepting for the 9 feet 6 inches by 9 feet 6 inches sewer, the concrete to be used throughout the works shall be Portland cement concrete, composed of clean Thames ballast or approved pit ballast, in the proportion of eight parts by measure of ballast to one part by measure of Portland cement."

[Mr. J. GRANT

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Mr. J. GRANT said, at this stage he would simply point out the form of moulds used in the experiments. No. 1, Table 1, was the first form, and was used previous to 1865. It would be perceived that the angles were very sharp, which was the cause of irregular fractures. After some experience the original form of mould was modified, as shown in No. 2, Table 1, in which the angles were rounded off, and which had been in use for several years. This was the form which had been used throughout the experiments described in this Paper, except where otherwise mentioned. At the same time other experiments, given in Table 1 of the Paper, were made with a variety of other moulds differing in length, shape and sectional area. Some, instead of being broken between clips, as shown in Plate 2 of his former Paper, were broken by means of knife edges inserted in holes, as shown in Plate 4, attached to that Paper. Further experiments were made with the fiddle-shaped mould, No. 10, Table 1, which did not break uniformly at the neck; the slightest distortion making it break in a diagonal line. Some other experiments were detailed in the Paper; but these were sufficient to explain their general character.

Mr. H. E. TOWLE said, in 1856 he was employed to take charge of works, where there had been a large quantity of the American Rosendale cement in store for a long time; and it became a question whether it should be used or rejected. It was determined that experiments should be made to ascertain the character of the cement, and some curious results were developed. This cement, in the United States, corresponded to the Portland cement of England. He tried the cement with various proportions of sand and lime paste, making up eight batches altogether. In making these experiments he desired to ascertain what effect working and reworking the mortar had upon its strength. He tried the mortar re-mixed daily for twenty days, and daily stuck bricks together transversely, and after five months pulled them apart, measuring the strain as carefully as possible. In these experiments he found it advisable to make a distinction between cohesion and adhesion. Cohesion was the quality which made the particles of a substance stick together between themselves; adhesion was the force which united the material to something else. Now, if bricks or stones were stuck together with mortar which did not adhere to the brick as strongly as it did to itself, all expense in making the component parts of that mortar more cohesive was wasted; and therefore there was a limit to the expense to be incurred for this purpose. Reverting to the re-mixing of the mortar, he found that the maximum strength was obtained after ten times mixing and re-

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mixing. He certainly did not advocate mixing and re-mixing twenty or any other number of times; nevertheless this fact about this particular kind of hydraulic mortar might serve as a suggestion to others endeavouring to render the subject less obscure. Newark Rosendale cement was sold in barrels of a capacity of 4 cubic feet, and each barrel contained on an average 300 lbs. of cement. The weight of the barrels when empty averaged 25 lbs. each. A weight of 300 lbs. of Rosendale cement required $1\frac{95}{100}$ cubic foot of water to reduce it to a stiff paste, and produced $3\frac{6}{10}$ cubic feet of pure cement mortar in a state of stiff paste, weighing 117 lbs. per cubic foot. The volume of 300 lbs. of cement unpressed was $4\frac{3}{4}$ cubic feet; and as sold in the barrels 4 cubic feet.

1 Barrel Rosendale cement . .	4 cubic feet.
1 Box lime paste, 2 feet by 2 feet by $\frac{3}{4}$ foot	3 „
5 Boxes sand, damp and loose . .	15 „
Total . .	22

produced a batch of 17 cubic feet of good mortar, weighing 124 lbs. per cubic foot:

Lime paste weighed, per cubic foot .	84 lbs.
Cement „ „ .	117 „

A block of unslaked lime 1 cubic foot in volume weighed $91\frac{43}{100}$ lbs., and when slaked by the addition of water, and reduced to a thick paste, the volume increased to 3·047 cubic feet. A bushel of this lime, as purchased coarse and fine together, weighed 75 lbs., and when reduced to a state of stiff paste increased in volume to $2\frac{193}{1000}$ bushels.

Lieut.-Colonel Scott, R.E., remarked that Mr. Grant had brought up the strength of Portland cement to a pitch which a few years ago was thought impossible; but he was not quite certain that in carrying it so far danger might not be incurred which at first sight might hardly be suspected. Persons who were in the habit of using large quantities of Portland cement were aware that at times it expanded to an extraordinary degree. This was due generally to too large a quantity of chalk having been used in its preparation. A few years ago, on examining some cases where great strength was aimed at by the manufacturer, he found that where the clay was reduced to 18 or 19 per cent. instead of 22 or 23 per cent. very extraordinary expansion occurred. By the exposure of such cement to the air, the tendency to expand might be, no doubt, greatly modified, but by demanding cement of extreme

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strength engineers undoubtedly ran into this danger. With a larger proportion of clay there was greater safety, though somewhat less strength. A remark had been made that it was of no use getting the cohesive strength of the cement greater than the adhesion of the cement to the materials that were to be joined together by it. If that were right, and in the use of Portland cement the strength of the bricks in ordinary use was exceeded, he thought engineers might be content to go no further and avoid the danger he had pointed out. As to the instance cited of cement mortar when beaten up afresh, though first losing strength, afterwards becoming stronger again, he thought it might be explained by assuming that the cement might have contained some particles which had an expansive tendency. In using hydraulic lime-mortar the lime was slaked, and thus was obtained a hydrate of lime mixed with sand, and in the work a solidification occurred, partly from the action of the atmosphere and partly from the slaked lime entering into fresh combination with the silex, thus forming slowly a hydrated silicate without expansion; but if particles of unslaked lime were mingled with this, inequality of action resulted, inasmuch as the particles of unslaked and slaked lime were in a different condition. In using lias lime, for instance, unless it was ground to a fine powder and the slaking allowed to go on some time before use, the work was apt to get distorted, because those particles which were uncombined with water expanded in the work when hydration took place and burst it. In the experiments alluded to he thought that something of this kind had occurred. There was continued expansion of some of the lime particles through slaking, till the material was brought wholly into the state of hydrated silicate of lime, when it again gained in strength by the quiet and gradual rearrangement of the particles, such as occurred with hydraulic limes, without any disturbing element.

He now would say a few words on a subject which merited attention from engineers. There was a new mode of using lime which had been adopted in the buildings at South Kensington. If in the ordinary slaking of lime just sufficient water was added to bring it to a dry impalpable powder, this powder would have a bulk sometimes much greater than that of the original lime, and if made into paste would, when it dried, exhibit a considerable degree of porosity. It could be imagined that if the lime, instead of being slaked, were ground to powder and induced to set as cement, with its original density, much greater strength would be attained in the solidified mortar. In the use of lime the combination with water took place first, and then subsequently the solidifying action

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went on. If it was hydraulic lime a silicating process was set up after slaking, and a fresh arrangement of particles took place. In the case of cements the slaking and the setting went on simultanously, and the water entered at once into combination with the cement to form a stone-like substance. If by any means lime could be induced to combine with the water slowly, so as not to burst with the heat, a much better result would follow than from the ordinary way of using it. This, in fact, was what he had accomplished by the mode in which he was now making mortar, and his general method of procedure was as follows, the lime used at Kensington being ordinary grey lime finely ground. First of all he mixed 5 per cent. of plaster of Paris, as compared with the quantity of lime to be used, in a bucket of water, making a milk of sulphate of lime. He then threw this into the pan of a mortar-mill and added the ground lime gradually, and instead of the lime slaking as it would when treated with water only, the lime showed no symptoms of slaking, and was brought to a thin paste without any sensible increase of temperature. When that was done he added sand, and for most purposes as much as 6 parts of sand to 1 of lime. In thin brickwork he obtained far greater strength with grey lime treated in this way, with 6 parts of sand, than with Portland cement with 4 parts of sand. He did not say the same results would follow the use of grey lime in very damp situations or under water; in such situations another set of phenomena came into play; but he spoke of cases where the carbonic action of the atmosphere was chiefly depended on to bring about the hardening of the mortar. The process he had described would not give setting property under water if the lime did not possess it before. It might indeed be thought that he destroyed the water-setting property by the plaster of Paris, which was itself soluble, but that was not the case. The quantity of plaster of Paris did not exceed 0·75 per cent. of the mortar; and in three or four weeks crystallised out in the pinhole cavities in it, where it showed itself in little crystalline spangles or plates. In proportion as the percentage of clay increased in the lime employed, so this became more suitable for mortar to be used under water. Puzzuolana was at one time largely imported into this country. At that time it was not known that there were large beds of a material in England which made excellent pozzuolana. He believed that with his process the puzzuolana of this country would come into use. He had examined the shales of the lias beds, notably those of Leicester, and he found by the use of this material calcined, and a small quantity of common chalk lime, treated as described with a milky solution of sulphate of

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lime, he got under certain conditions, viz., by experiments with small blocks and by putting a few bricks together, greater strength than he could get out of Portland cement. He spoke of such instances as those mentioned only. His experiments had so far extended only to such cases. He had only to add that in using this new method of mortar-making (which he termed the selenitic method) for plastering the walls in the galleries of the Exhibition Building at South Kensington, he employed 6 parts of sand to 1 part of lime, and for the finishing coat 1 part of lime with 4 parts of sand.

Mr. BRAMWELL said that when, five years ago, Mr. Grant brought before the Institution a Paper on this subject,[1] and pointed strongly towards the improvement that he thought would be derived from increasing the weight of the cement, Mr. Bramwell threw doubts upon the probability of such a result, since, from experiments he had made, he believed, if the weight of the cement were alone considered, it would hold out a premium for bad grinding, and that badly-ground cement meant inferior cement. He was glad to find this opinion corroborated in the present Paper: for Mr. Grant now said that, with cement of 113 lbs. per bushel when unsifted, he got a strength of 375 lbs. per square inch, but when sifted the same cement, weighing only 110½ lbs. per bushel, gave an increase of strength reaching to as much as 427 lbs. He thought this abundantly justified what he stated five years ago, that the mere test of weight alone was a premium for bad grinding, and calculated to mislead. He also, at the same time, gave instances in which large variation of strength was occasioned by an extremely small variation in the amount of water with which the cement used in the sample was gauged. He was glad now to hear Mr. Grant state that the proportions, both of water and cement, were determined by weight in the mixing of the samples tested.

Mr. BAZALGETTE remarked that he could not understand how it was possible to manufacture cement of too good a quality. The improved manufacture of Portland cement up to the present time had been promoted by the careful experiments of Mr. Grant. Portland cement concrete could now be used with advantage and safety where brickwork and stonework were formerly used, thus effecting a large economy in engineering works. In the Southern Thames Embankment the cost was reduced to nearly one-third of what it would have been had the embankment continued to be constructed in brickwork behind the granite facing. A further

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxv., p. 136 *et seq.*

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portion of embankment was on the point of commencement on the north side of the river, and this also it was intended to construct with granite facing and concrete backing without any brickwork, which he believed, owing to the good quality at which Portland cement had now arrived, would be quite as strong, and probably would form a more homogeneous mass than if constructed in brick work. The Paper had drawn attention to the fact that Roman cement was two-thirds the price of Portland cement, but that its strength was only one-third; therefore it was more advantageous to use Portland than Roman cement. He did not notice whether the Author had made any comparison between Portland cement and lias lime mortar; but as far as his experience went, Portland cement was so superior to the ordinary limes used for mortar in brickwork that the cement became the cheaper material of the two. For instance, he had in his experiments added in bulk the quantity of lime required to bring it up to the same price as a less quantity of Portland cement; and he found, using it in that way in mortar, that Portland cement produced much stronger brickwork than was produced by the use of the proportionately increased quantity of lime.

He did not clearly understand the distinction drawn in the course of the discussion between adhesion and cohesion. As he understood the argument, it was contended that, with harder and better cement, its power of adhering to stone or bricks would not increase in proportion to its increased power of holding together its own particles in one mass. He was, however, of opinion that if this argument had reference to cement uniting brickwork or stone, it would be found if the face of the materials to be united were rough and absorbent, the cement could penetrate the face of the brick or stone, combine with and lay hold of it, and have the same affinity to the brick or stone as to itself; and that this power of combining the whole mass together would under such conditions increase proportionately with the increased strength of the cement, so that the adhesion and the cohesion would increase at the same time and in the same proportion. He thought therefore if the strength of the cement was increased, and the building materials to be united were suitable, both the adhesion and the cohesion would also be increased, and the whole structure strengthened. The Paper led to this practical result—that by attention to the production of good Portland cement the cost of engineering works might be reduced, particularly those of large bulk, to less than half that which had been spent upon similar works before its introduction.

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Mr. W. PARKES said he had lately had the opportunity of making some experiments with Portland cement, from which he had drawn conclusions similar to those of Mr. Bramwell. Every one who had to do with Portland cement must feel indebted to Mr. Grant for the attention he had given to the subject and the results he had made public; but though the former Paper was of great use, he found the conclusions given by Mr. Grant were also liable to great abuse. Those conclusions were apparently so simple that people who were not altogether well versed in the subject were apt to suppose that they could test cement, and know good from bad by the simple application of the rules. He concurred with Mr. Grant that two of the qualities of good cement were high specific gravity and high tensile strength; but he thought it would be inexpedient to push those principles to extremes. Good cement was generally heavy and strong, but it did not always follow that the quality was proportionate to the weight, or to the tensile strength, or to the two combined. In the case of some Portland cement, lately required for sea-work in India, the Government had, under his advice, applied for tenders from several manufacturers of repute, and the three lowest tenders were accepted. It happened that the cement supplied by those three makers differed, to some extent, in the points under consideration. One being a heavy cement weighing about 120 lbs. to the bushel, equal to a tensile strain of 800 lbs. to 1000 lbs., on a sectional area of 2·25 square inches; another, weighing about 108 lbs. to the bushel, which bore a strain of 600 lbs. or 700 lbs.; and the third was intermediate between the two in weight, viz., about 114 lbs. to the bushel; but the tensile strength was, on the whole, greater than that of the heavier cement. The superintendent of the works was, in the first instance, favourably disposed towards the heavier cement, with high tensile strain; but after testing it, by exposing blocks of concrete to the action of the sea during a monsoon, he was unable to say that one cement was practically better than the other, as judged by this test. Subsequently he had the means of applying another practical test, by cutting into the blocks. According to this test, he gave the preference to the first heavy cement, as being decidedly harder than the other two. As to the two others, he could not say which was the best. But, on the whole, there was probably no great practical difference between the three cements.

He would mention another fact, to show that weight and tensile strength did not always go together. A firm of cement manufacturers requested him to test some cement which had been condemned, and he applied the ordinary tests to it. In the first

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place, he weighed it, and found the weight to be 101½ lbs. to the bushel. This was very light, and he expected to find a very weak cement; he noticed, however, one remarkable fact in making up the test blocks. He adopted the plan, which Mr. Grant now recommended, of using exact proportions of cement and water; but he measured the cement and the water, instead of weighing them. With this light cement he found this curious result. The measure he adopted had a capacity about 50 per cent. greater than that of the test-block mould, and this quantity of heavy cement in powder was sufficient, when worked up with water, to fill the mould, and even left a little excess after the mould was filled; but with the light cement it was not enough; the contraction of bulk was greater, so that it required a larger measure of that light cement to fill the mould than it did of the heavier material. When he broke the test-blocks he was surprised at the strength—the several breaking strains were 680 lbs., 600 lbs., 600 lbs., 585 lbs., and these did not give the full strength, for they were taken out of the mould while soft. Two other samples, which were kept in the mould till quite hard, were equal respectively to 960 lbs. and 840 lbs., which, for cement of only 101 lbs. to the bushel, was, he thought, very high indeed. Now, if the virtue of that cement was in proportion to its tensile strength, it was not in proportion to the weight.

He was glad to find the Author now insisted that extreme care was necessary in making these tests. Generally speaking, the makers tested their cement at their own works, and employed for that purpose a man called a sampler, who had little else to do but test the cement as it was made; and those men, from long practice, had acquired so much skill that, though they performed the operation in a very rough manner, they obtained pretty regular results. But when the cement was sent to the consumer, and the consumer wished to test it for himself, he necessarily employed a person of less skill to make up the test-blocks, and it was almost impossible to get consistent results. There would sometimes be a difference of 100 per cent. or 150 per cent., without any apparent cause, in the breaking strain of two bricks made from the same sample. Consistent results could only be obtained by adopting exact proportions of cement and water. He had made some experiments to illustrate this. In one experiment, he used $\frac{4}{10}$ths of a pint of water for each test-block, when the breaking weight in four cases was 970 lbs., 980 lbs., 1000 lbs., and 980 lbs. After that, he made up two more samples, mixed with ½ pint of water, instead of $\frac{4}{10}$ths of a pint, and those broke with 590 lbs. and 370 lbs. Two

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others, made up with $\frac{9}{20}$ths of a pint, broke with 750 lbs. and 375 lbs. Other experiments, giving similar results, led to the conclusion that it was most important to reduce the quantity of water to a minimum, and for this purpose to have it accurately measured. He would mention a case in which neglect of this precaution had nearly led to serious results. Part of the cement which he sent out to India had been tested by himself before shipment, and the samples broke at a little over 800 lbs. When the cement arrived at its destination, the works had been stopped for a time, and 400 tons of cement were left on hand. This was transferred to another work at Bombay, and when it arrived there the Engineer who was to employ it was directed to test the cement. The tests were made, and the results were given in a strongly-worded despatch to the India Office, stating that the cement was good for nothing; inasmuch as the tests gave as its tensile strength only 248 lbs. on the brick; and that its employment in the work would not be permitted. He thereupon recommended that some samples of the cement should be sent home for further tests. On its arrival it was found to be damaged by damp, to which it had been naturally exposed in its three voyages and various transhipments; nevertheless, the test-blocks made from it bore strains of 660 lbs., 690 lbs., and 800 lbs., showing that there was no great deterioration of the cement. There could be no doubt that the tests at Bombay were not made with the necessary precautions; but he was convinced that there was at present a very prevalent ignorance as to their necessity, which he hoped would be removed now that Mr. Grant had brought the weight of his authority to insist upon them. Properly employed the tests were most valuable, but in the hands of inexperienced persons they were worse than useless. He would also express his opinion, that it was a mistaken practice to record only average results. All records should give individual results, by which alone the value of the tests could be judged.

The weight test was one which also required to be applied with due precaution. The rough mode employed by the makers of shovelling the material into a bushel measure was safe enough if always carried out by the same person, and he a careful and experienced one; but it was possible, by an almost involuntary increase of force, to increase the density 2 or 3 per cent., and by an increase such as could not be prevented by a specification, to increase it 10 or 12 per cent. The employment of a hopper, as described by Mr. Grant, was the best precaution against this source of error.

There was another point, however, in which care was required.

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He had alluded to some very light cement on which he had experimented. The makers had assured him that this cement, when manufactured, was of much greater density. This led him to experiment on other samples; and he found in four cases that cement taken fresh from the manufactory or from the cask was heavier by respectively 6½ lbs., 7 lbs., 4½ lbs., and 2¾ lbs. per bushel than it was after two or three days' exposure to the air; though in this latter state it was in better condition for employment in practical work. It was, therefore, necessary to specify that the density should be determined when the cement was fresh from the manufactory, and not at any subsequent time.

Mr. G. F. WHITE observed that, as one of a numerous body of cement manufacturers, he desired to express his obligations to Mr. Grant for the continuance of his investigations into the subject of Portland cement. It was needless to say that the manufacturers themselves were anxious and critical investigators of the products they turned out, and had their attention constantly directed to their improvement. He might say that in his own works there were recorded thousands of experiments for years past on the strength of cement; but he could well understand that no trials made by a manufacturer on his own material could have for the members of the Institution the same value as those conducted by an independent engineer. His own observations on the subject of Roman cement enabled him to corroborate the remarks of Mr. Grant as to that material. It was the fact that even after five years this cement did not attain to a greater tensile strength than 100 to 120 lbs. on the square inch, which was less than a third of the strength of Portland cement. This sufficiently accounted for what was at one time a large manufacture having dwindled down to nothing, Portland cement having entirely superseded it. The same remark would, in great measure, apply to Medina cement, which was but a variety of Roman cement with rather more lime in it; and which, though valuable for under-water work, where rapid setting was indispensable, had on the other hand the disadvantage which attached to all quickly crystallizing cements of attaining to only a moderate degree of ultimate hardness. He fully agreed with the Author of the Paper as to the superiority of the moulds made with curved instead of straight shoulders, as tending to reduce the inequality of the strain, and to bring the pressure of the lever more equally on the centre of the brick to be tested. With respect to the wide differences of the results obtained by different experimenters on the same cement, there was no doubt that different modes of gauging were adopted by different manipulators, some using more and

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others less water to mix the cement. Something too might be due, in the case of long voyages, to the cement having become slow setting from staleness; but the mode of testing and the machine used was at the bottom of the difficulty, and it would be highly desirable that some rule of universal application should be adopted in order to put an end to these differences. Persons who had used Portland cement for the arches of tunnels or bridges would have noticed the milky exudation referred to by Mr. Grant, in cases where there was much moisture above the arch. The appearance, indeed, was not confined to cement but often showed itself where lime mortar was present. He considered that the milk of lime so formed was, in both cases, attributable to the mechanical action of the moisture, which released a minute quantity of the uncombined lime, and caused it to exude. If there had been an excess of lime, in the sense intended by Mr. Grant, the water would have slaked it and ruptured the brickwork; but he gathered that no bad results had followed these appearances. It was, however, a subject well worthy of further investigation. Concurring as he did in the main with Mr. Grant's conclusions, that concentration of burning and tensile strength had a direct and significant relation to one another, he still maintained that, inasmuch as cement weighing 120 and 125 lbs. to the bushel could only be obtained by the admixture of excessive proportions of lime, there was always the liability to disintegration of the mortar if any of the lime were in a free state. He could not avoid noticing the fact that the strength of the mortar made with the heavy cement did not bear the same relation to mortar made of the light cement as the two cements did to one another when mixed pure. This seemed to show that it would be fairer to test the cement with, say, 2 parts of sand in the way it was going to be used in the work, after the manner usually adopted by the French engineers. If Mr. Grant, in addition to the other services he had rendered, could point out how good cement could be made with less fuel, and thus lighten a burden which was at present their *bête noir*, he would be conferring a lasting obligation on cement manufacturers in helping them to cheapen the cost and improve the quality of their fabrication at one and the same time.

Mr. J. A. M'Connochie inquired whether in Mr. Parkes' experiments, where unequal quantities of water were used in mixing the test-blocks, the time allowed for setting was the same in each case? It appeared to him that the time allowed for setting should be regulated by the quantity of water used, if a comparison were to be drawn from the experiments as to the relative ultimate strength

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of the cement. He recently had occasion to send several thousand tons of Portland cement to Bombay for the drainage works, and in the case of one shipment a similar report reached him as that referred to by Mr. Parkes, that the cement on its arrival did not stand the tests it had been subjected to before it left this country. He believed that this resulted from the cement having been put into casks before it had sufficiently cooled, and that it had consequently deteriorated during the voyage. This was of great importance in sending Portland cement to hot climates, and on its arrival it should be removed from the casks, spread out, and turned over to cool.

The result of an extended series of tests which he had made entirely confirmed Mr. Grant's conclusions, that the two important features to be sought for in Portland cement were weight and fineness; and that as cement possessing these properties was slower in setting, tests at a longer period than seven days were more satisfactory where they could be obtained, for although several samples of unequal weights might attain nearly the same strength in the seven days' test, the heavier and slower setting would ultimately be decidedly the stronger.

He had adopted a different breaking section in his test-moulds from that of Mr. Grant, retaining the same sectional area of 2·25 square inches, but disposed in a depth of 2·25 inches by 1 inch wide, in place of 1½ inch square. He found that with this section the blocks broke in nearly all cases in the smallest section, and not at the shoulders, as so often happened even with Mr. Grant's improved pattern.

He was surprised to find from Mr. Grant's experiments that cement mixed by hand was double the strength of that ground in a mortar-mill. He believed that this must be due to the materials having been mixed for an excessive time (thirty minutes) in the mill; if six or seven minutes were tried he expected the result would be reversed, and that the cement ground in the mill would be superior to hand-mixed.

In the case of a dock wall recently built at the Surrey Commercial Docks, he specified Portland cement for concrete block facing to weigh 116 lbs. to the bushel, with a breaking weight of 350 lbs. per square inch at seven days in water. The contractors delivered a cement procured from Messrs. Lee, Son and Co. exceeding the specification, and weighing 120 lbs. to the bushel, with a breaking weight of 440 lbs. per square inch at seven days. Finding this to be the case he abandoned blue lias lime for the concrete backing, and substituted Portland cement concrete mixed in the propor-

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tion of 1 of cement to 12 of sand and gravel. This proved entirely satisfactory. He mentioned this as an illustration to show that the idea which had been advanced, that it was unnecessary to seek for a cement stronger than the materials to be cemented, was, even on the ground of economy, an erroneous one, as the additional cost per ton of a heavy and strong over a light and weak cement was trifling compared with the saving resulting from being able to use a greater quantity of sand with it. A valuable property of Portland cement, which had not been alluded to, was that the work executed with it scarcely ever suffered from frost.

It was to be regretted that, with one or two exceptions, the rule of thumb still so largely prevailed in the manufacture of Portland cement, and he believed that by raising the standard of quality still higher than Mr. Grant had yet done, the manufacture would be improved and cement more uniform in quality be produced.

Mr. W. Parkes replied that in all his experiments the time allowed for the cement to set had been uniformly seven days.

Lieutenant W. Innes, R.E., described some experiments on which he had been engaged. The cement used was sampled out of a large quantity supplied for War Department Works, and was of a high average weight and tensile strength. Weighed in the ordinary manner it would have been from 118 lbs. to 120 lbs. per bushel; the weights given in the following tables were lower, having been got by sliding the cement into the measure down a piece of board. Each result given was the mean of not less than ten experiments, all due precautions having been taken with the manipulation, and the specimens broken by tension in one of Adie's machines, similar to those used by the Metropolitan Board of Works.

Table I. (p. 320) showed the tensile strength at three weeks and three months of mortars composed of cement with two parts of seven different substances, including four varieties of sand, stone dust, clay ballast, and smiths' ashes. The whole of the sands, &c., had the grains above $\frac{1}{12}$th of an inch in diameter sifted out before use, to avoid the uncertainty caused by larger grains in results got from a section only $1\frac{1}{2}$ inch square. The proportion of each which would have been rejected by a sieve of 2,500 meshes per square inch was inserted, to give some idea of the different degrees of fineness, but what was actually used was not so sifted. *Cæteris paribus*, coarse sand gave better results than fine, both as regards strength and bulk of mortar produced (*a* and *b*, Table I.); and of mortars attaining nearly the same ultimate strength, that made with the finest sand set the slowest (*b* and *e*, Table I.). The great

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TABLE I.

GIVING RESULTS of EXPERIMENTS on MORTARS of PORTLAND CEMENT and Various Kinds of SAND, &c., made with one measure of the former to two of the latter.

	Sands, &c. Physical Characteristics.				100 Parts of Mortar contained			Ultimate Tensile Strength.				Remarks.
								At Three Weeks' Immersion.		At Three Months' Immersion.		
	Per cent. of Voids dry.	Per cent. of Voids wet.	Shrinkage on wetting.	Per cent. above $\frac{1}{50}$ inch diameter	Cement.	Sand	Water.	Lbs. per Sq. Inch.	Proportional.	Lbs. per Sq. Inch.	Proportional.	
	Neat cement.			31	141	..	39	450	1·00	529	1·00	Inserted for comparison with the other mortars.
a	38	34	6	94	38	76	18	140	·31	249	·47	A sea-sand, with roughish and uneven grain, chiefly silicious and quite clean. Mortar very short and incoherent; strength of different samples somewhat variable.
b	41	34	11	76	40	80	24	94	·21	175	·33	A silicious pit-sand, quite clean, with smooth, uniform, semi-transparent grains. Mortar somewhat "short," strength very uniform.
c	32	19	16	15	40	80	34	108	·24	248	·47	A silicious pit-sand, containing a number of minute shells and a small quantity of some orange-colouring matter; grains semi-transparent, brownish yellow, and of unequal size. Mortar rather "short;" strength tolerably uniform.
d	46	34	18	56	42	84	22	165	·37	254	·48	Portland stone dust, a mixture of roach and whitbed; grains rough and irregular, quite clean. Mortar tolerably coherent; strength very uniform.
e	43	36	11	8	39	78	31	60	·13	193	·36	Drifted sea-sand, with pure silicious, semi-transparent, and almost colourless grains, and quite clean. Mortar rather short; strength tolerably uniform.
f	64	52	25	56	44	88	44	38	·08	91	·17	Smiths' ashes, containing, however, a good deal of unburnt coal-dust; grain rough and irregular. Mortar moderately coherent, set slowly, and apt to shrink and crack; strength very uniform.
g	50	40	17	40	48	96	39	269	·59	355	·67	Clay ballast, burnt and ground, of a pale brick-red colour, with a rough uneven grain, and containing a good deal of dust. Mortar very coherent, apt to shrink and crack; strength very uniform.

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difference seemingly caused by a small mixture of minute shells was very noticeable. The high results obtained with burnt clay (*g*) might probably be due to its acting as a puzzuolana with the free lime in the cement. Mortar made with this substance was very coherent, and resisted the washing action of water when green better than any other; but the bulk of mortar produced from a given quantity of material was very small, and it had the disadvantage of cracking and shrinking if allowed to dry before it was well set. The low result got with the coal ashes was unexpected, and might have been due to its containing a good deal of unburnt coal dust; this admixture however was not uncommon.

The two remaining tables showed the results of experiments on the effect of coarseness of grain on the properties of cement.

Table II. (p. 322) showed the strength at three and six months, and with from 0 to 3 parts of sand, of mortars made from cement of three degrees of fineness, viz., as received from the manufacturer, and the same after being passed through sieves of 1,300 and 2,500 meshes per square inch, which rejected about 20 and 30 per cent. respectively. The weight per bushel and the strength of the neat cement were both diminished by sifting, the coarsest being heaviest and strongest and the finest lightest and weakest; but when gauged with one or more parts of sand the order of strength was reversed, the fine cement giving the best results. With three parts of sand and at six months the finest cement attained ·47, whilst the coarsest attained only ·29 of their respective strengths neat at the same age.

Table III. (p. 323) showed the strength at three and six months of neat cement varying in fineness from what would all pass through a sieve of 2,500 meshes per square inch down to that of which 70 per cent. would be rejected. The same cement was used for all, being first sifted and then remixed in the proportions given. At three months the strength increased with the quantity of coarse grains up to 40 per cent. of the latter; when the cement contained more than this proportion the results, though high, were variable; at six months the strength increased with the quantity of coarse grains up to the highest proportion tried, viz., 70 per cent. Had it not been expected that the maximum strength would have been reached with a much smaller or no proportion of coarse grains higher proportions would have been tried, but there would have been considerable difficulty in gauging the coarse cement freed or nearly so from the fine, as it was as harsh and incoherent as so much gravel.

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TABLE II.

RESULTS of EXPERIMENTS on the effect of the Coarse Particles ($\frac{1}{50}$th of an inch and upwards in diameter) of heavy PORTLAND CEMENT.

Quality of Cement used.		Ultimate Tensile Strength at Three Months.								Ultimate Tensile Strength at Six Months.							
		Net.		1 Sand.[2] 1 Cement.		2 Sand.[2] 1 Cement.		3 Sand.[2] 1 Cement.		Net.		1 Sand.[2] 1 Cement.		2 Sand.[2] 1 Cement.		3 Sand.[2] 1 Cement.	
Fineness, &c.	Weight per Bushel.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.	Lbs. per Inch.	Proportion.
	lbs.																
Cement as received from the manufacturers	116	504	1·00	286	·57	117	·35	103	·20	527	1·00	350	·66	229	·43	151	·29
The same passed through a sieve of 1,296 meshes per square inch which rejected 20 per cent.[1]	162	457	1·00	324	·71	212	·46	143	·31	492	1·00	424	·86	281	·57	173	·35
The same passed through a sieve of 2,500 meshes per square inch which rejected 30 per cent.[1]	99	449	1·00	377	·84	250	·56	173	·39	486	1·00	439	·90	323	·66	228	·47

REMARKS.—The finest cement set quickest, required most water in gauging, and gave the most coherent mortar: it gave the smallest bulk of mortar from a given quantity of materials.

[1] These percentages are given by weight.

[2] The proportions were measured in light dry powder.

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TABLE III.

RESULTS of EXPERIMENTS on the effect of the Coarse Particles ($\frac{1}{50}$th of an inch and upwards in diameter) of heavy PORTLAND CEMENT.

Quality of Cement used	Ultimate Tensile Strength in lbs. per Square Inch.		Remarks.
	Kept in Water Three Months.	Kept in Water Six Months.	
Neat cement, of which none was rejected by a sieve of 2,500 meshes per square inch	464	492	The same cement was used for the whole series, being first all sifted, and then remixed in the proportions shown. As received from the manufacturer it weighed about 117 lbs. per bushel, and contained more than 30 per cent. which failed to pass a sieve of 2,500 meshes per square inch. The percentages are all given by weight. The ultimate chemical composition of the fine and coarse grains was almost identical.
Ditto 10 per cent. ditto ditto	482	508	
Ditto 20 ,, ditto ditto	499	510	
Ditto 30 ,, ditto ditto	507	515	
Ditto 40 ,, ditto ditto	500	520	
Ditto 50 ,, ditto ditto	490	525	
Ditto 60 ,, ditto ditto	512	527	
Ditto 70 ,, ditto ditto	489	535	

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Supposing the results given in Tables II. and III. to be trustworthy, they would seem to indicate a necessity for modifying the present weight test and adding one for fineness, for the heaviest cements were also the coarsest (heavy cement often contained 30 per cent. of grains upwards of $\frac{1}{50}$th of an inch in diameter), and coarse cement, though it gave a high tensile strength neat, did not bear sand well; so that a cement giving good results with the usual weight and strength tests might give a weaker mortar when used (as it almost always was) with sand, than one not passing the tests so well. This had an important bearing on what had been remarked on as to the doubtful advantage of a cement stronger than the building material; the advantage of such a cement should be its capacity for sand, but much of this was lost by coarseness of grain.

A specific gravity test would probably be better than the common weight test, if some convenient method of applying it were devised; or if the weight test were retained it might be applied to a sample passed through a sieve of some fixed degree of fineness; the standard would probably in such a case have to be reduced, as it seemed doubtful whether it would be possible to manufacture a fine cement of the high weight per bushel now thought necessary, though of equally fine cements the heaviest would doubtless be the best.

Mr. G. H. Phipps said he could not subscribe to the argument, that there was no use in having a cement stronger than the materials to be held together, as he thought there were many cases where a material possessing that property was exceedingly useful. For instance, in brick abutments for arched bridges it was always an important matter to secure the courses from sliding horizontally upon each other at the base of the skewbacks at the springing of the arch. This, he thought, could be better secured with a cement of high cohesive power, as when considered in relation to the cross joints, as well as the horizontal ones, any fracture had to pass through a considerable quantity of the cement itself, as well as to strip it off the bricks. He thought it a great advantage to be possessed of a material combining the properties of great adhesion and cohesion, and which at the same time when reduced in strength by various admixtures of sand, could be brought down nearly to the cost of common mortar.

Mr. B. Latham observed, through the Secretary, that in considering the question of the strength and durability of cement or mortar the attention of engineers should be directed not only to the necessity of providing a material of great cohesive strength, but also one

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which, when applied in certain positions, should resist the influence of certain chemical forces tending to destroy it, when the inevitable result would be a failure in the work executed. During the last two or three years he had found that sewers which had been constructed for about seventeen or eighteen years, the brickwork of which was set in various kinds of lime and cement, and in some cases in a mixture of both, were failing, and many yards of such sewers had fallen in. On examination of these defective sewers he had found that every particle of cement or lime which had been used in the mortar had disappeared from the joints, leaving nothing but the sand to hold the work together. This failure was due to the fact that sewer-water contains a certain percentage of ammonia; the ammonia, when brought into contact with ordinary lime, was converted into nitrous and nitric acid, which, in its turn, attacked the lime, forming nitrates and nitrites, which were extremely soluble, and which were washed out either by the sewage or by the infiltration of subsoil water into the sewers, leaving nothing but the sand in the joints. Cements and limes, intended to be used in such a position as to be exposed to chemical influence, should, in addition to the tests recommended by Mr. Grant, be subjected to a standard solution of nitric acid, in order to ascertain whether or not the material was suitable for the particular class of work for which it was required. Portland cement was the best material for resisting the chemical action of sewage; on the other hand, lime made from chalk was the worst. Therefore there was a possibility of carrying the manufacture of Portland cement to such a point that, in order to secure a great tensile strength, so much chalk might be used in its manufacture as to render it unsuitable to resist the chemical action of the matters with which it might be brought into contact. He had some years ago adopted the mode of constructing sewers recommended by Mr. Grant, viz., with an internal ring of brickwork, the external portion of the sewer being formed in concrete; and during the last six or seven years he had constructed several miles of such sewers, and could bear testimony not only to their efficiency, but to their great strength and durability, and to the cheapness of their construction. In the case of a sewer at Croydon, which was 4 feet in internal diameter, the inner circle being formed of a 4½-inch ring of brickwork, and the external portion of the sewer consisting of 6 inches of concrete, the sewer was constructed at 40 per cent. less cost than would have been the case had the whole work been executed in 9-inch brickwork.

Mr. J. GRANT, in reply upon the discussion, observed that

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Mr. Towle, Colonel Scott, and others contended that it did not follow that cement of great cohesive was necessarily of great adhesive strength; in fact, that no advantage was gained by making cement of a strength exceeding that of the materials to be cemented together. It was with the object of settling this point that the B Series of experiments was made. In the tables relating to that series it would be seen that the cohesive varied directly as the adhesive strength of cement; that in fact neat cement was from three to four times the adhesive strength of any mixture of it with four or five times its bulk of sand; and on this account he was prepared to state, in general terms, that cohesive and adhesive strength might be taken as equivalent to each other. For the purpose of cementing together various materials, he need hardly point out that to have strong cement or mortar was not the only thing necessary. For instance, two cubes of glass could not be cemented together, however strong the cement, so that they should adhere with the same tenacity as two bricks or blocks of stone of a porous character, and rough on their faces. It would not be possible, for example, to take such bricks as were recently used for lining the subway from the Houses of Parliament to the adjoining railway station, and to cement their highly glazed china faces together so that it would require the same pull to separate them as it would to separate two stocks, wire cut gault, Fareham red, or other porous bricks, capable of absorbing about $\frac{1}{5}$th part of their weight of water. Whilst on this point he might state that only second in importance, if second, to the strength of the cement was that of seeing that the bricks or stone to be cemented together were thoroughly saturated with water. In hot climates, like that of India, any work being built with cement should be kept wet, and if possible under shelter during its progress and for some time afterwards. Two bricks might be put together dry as they came from the kiln with the strongest cement that could be made, and if not previously soaked in water, these bricks might be separated without the least difficulty, the joint being very little better than if made of dry sand. With bricks which absorbed from 1 lb. to 1½ lb. of water, it was evident that if left to absorb this moisture from the mortar with which they were cemented they took away that which was necessary for crystallizing or setting the cement.

He thought it must be self-evident that, for making concrete, strong cement was much to be preferred to weak. Concrete made of absorbent material, such as broken bricks, stone, or pottery, was much stronger than concrete made of Thames

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ballast or any other slightly absorbent material. In the D Series of tables it would be found that concrete made of Thames ballast was only from half to two-thirds the strength of concrete made of broken bricks, stone, or pottery. It would be found also that bricks of a somewhat porous character, like the red bricks or the wire-cut gault bricks, and the other varieties shown in Table B, adhered together with a strength double that of bricks like the Staffordshire blue bricks without a frog, or bricks that had a smooth or glazed surface.

On the point of strength it had been pointed out clearly five years ago, and again on the present occasion, that the strength of cement could always be reduced by mixing sand in any proportion.

With respect to the cement or lime mentioned by Colonel Scott, he had the pleasure of going over part of the works at South Kensington, and had also an opportunity of testing the cement in one of the large sewers where it was used in the arch; but he was sorry to say it did not answer the purpose, and was much inferior to lias lime. On a later occasion he made a number of briquettes or moulds of it, as in the case of cement; but it was so weak that it would not bear winding up in the machine. Thus either Colonel Scott was not fortunate enough to have made his comparisons with Portland cement of a good quality, or he had himself been unfortunate enough to meet with an inferior sample of the lime or cement referred to. The objections raised by Mr. Parkes as to the comparative strength of cements of different specific gravities might be valid if the time allowed for the heavier cement was short; in fact, the heavier the cement the longer time it was necessary to give it to set. For experimental purposes, with heavy cements, a month was fairer than a week. For practical purposes, in works carried on in the streets of London, a week was a more convenient time; but for experimental purposes, with different weights of cement, the longer time was much more satisfactory.

As to the strength of cements generally, if it could be made of a high quality it might at any time, as he had stated, be reduced in strength. It was not unlikely at some future, though it might be far distant, time, that many of the sandy districts of Africa and other parts of the world might be irrigated by aqueducts, for the construction of which all the materials necessary would be comparatively small quantities of cement in a concentrated form, to reduce the bulk and cost of freight, mixed with the sand upon the spot.

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Mr. VIGNOLES, President, said there was not time for him to express fully his individual opinion on this subject, and he must content himself with observing that he did not concur in all that Mr. Bazalgette and Mr. Grant had propounded, and that there was much to be said upon what had been advanced by Colonel Scott with regard to having too strong an adhesive material. He thought the subject had yet to be considered more fully.

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May 2, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

The following Candidates were balloted for and duly elected:—WILLIAM JOHN BIRD CLERKE, A.B., LOUIS JOSEPH AIMÉ THOMÉ DE GAMOND, and ROBERT PIERCY, as Members; HERBERT CHAPMAN, GEORGE COOPER, HENRY TANNER FERGUSON, DRUITT HALPIN, HODGSON MONTEITH LAYARD JONES, CHARLES FERDINAND DE KIERZKOWSKI, WALTER HENRY MAUDSLAY, JAMES RICE, and JOHN LIDSTONE WATTS, Capt., R.E.

No. 1,299.—"Description of Two Blast Furnaces erected in 1870 at Newport, near Middlesbrough."[1] By BERNHARD SAMUELSON, M.P., M. Inst. C.E.

THE constantly increasing demand for crude cast, or pig iron, as the basis of all iron manufactures and constructions, and the efforts made to diminish its cost, have reacted on each other, until the production of pig iron in England, Wales, and Scotland in 1870 exceeded 5½ millions of tons, and will in the present year probably not fall far short of 6 millions of tons, or more than double that of seventeen years ago, when the great iron industries of Cleveland and of Cumberland were still in their infancy; four times that of 1840, and three-hundredfold that of 1750, when the Sheffield tanners petitioned Parliament to discourage the importation of American iron, lest the Yorkshire furnaces should be extinguished, the wood used by them be no longer cut, and consequently the supply of bark for the tan-pits should not be forthcoming.

The blast furnaces of those days produced, on an average, 300 tons of pig iron yearly. Each of those about to be described yields 24,000 tons in the same time. That is to say, the produce of a single modern blast furnace is equal to nearly one and a half time all the crude iron made in this country one hundred and twenty years ago.

The process of smelting the ores of iron is well known. The

[1] The discussion upon this Paper extended over portions of two evenings, but an abstract of the whole is given consecutively.

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ores, the fuel, and the flux are charged at the top of the furnace, in proportions varying according to their chemical constituents and mechanical condition, and according to the greater or less perfection of the furnace and of its accessories. Air is blown in through tuyeres at the hearth, scoria flows over the dam, and the iron is allowed to run from the tapping hole at proper intervals. Economy in smelting is sought in a diminished expenditure of fuel and flux, and of labour in producing a ton of iron from a given weight of iron ore, and is promoted, in the first place, in all ores containing much moisture or carbonic acid, or both, by their perfect calcination, before they are 'charged' in the furnace. Secondly, by the proper coking of the coal, where it is too bituminous, or too liable to decrepitate, to be used raw. Thirdly, by heating the air before it is introduced into the furnace, so that a very high temperature may be created in the zone of fusion, immediately above the tuyeres. And fourthly, by durability and simplicity of construction of the entire plant, in order to insure the utmost constancy and regularity in working.

The iron stone smelted in Cleveland is the argillaceous ore from the lias, containing, when dried, from 33 to 40 per cent. of protoxide of iron, and from 2 to 7 per cent. of sesquioxide of iron, equal to 26 to 33 per cent. of metallic iron, which percentage is increased in calcined ironstone to from 37 to 40 per cent. by the moisture and part of the carbonic acid being removed by calcination.

The raw stone also contains

From	20 to 25	per cent.	of carbonic acid.
„	3 to 4	„	magnesia.
„	5 to 8	„	lime.
„	10 to 15	„	silica.
„	10 to 15	„	alumina, and
„	1 to 1½	„	phosphoric acid.

The fuel is the coke of South Durham, yielding

From	85 to 92	per cent.	of carbon.
„	½ to 2	„	sulphur, and
„	4 to 12	„	ash.

The flux is raw or burnt limestone, chiefly from the Pennine range, containing, in the raw state, from 87 to 96 per cent. of carbonate of lime.

The fuel and limestone consumed, in order to produce a ton of grey foundry pig iron from Cleveland ironstone, without admixture of foreign ores or of the cinder from rolling mills, are from 19 to 28 cwt. of coke, and from 10 to 14 cwt. of limestone, the figures varying with the fusibility and richness of the

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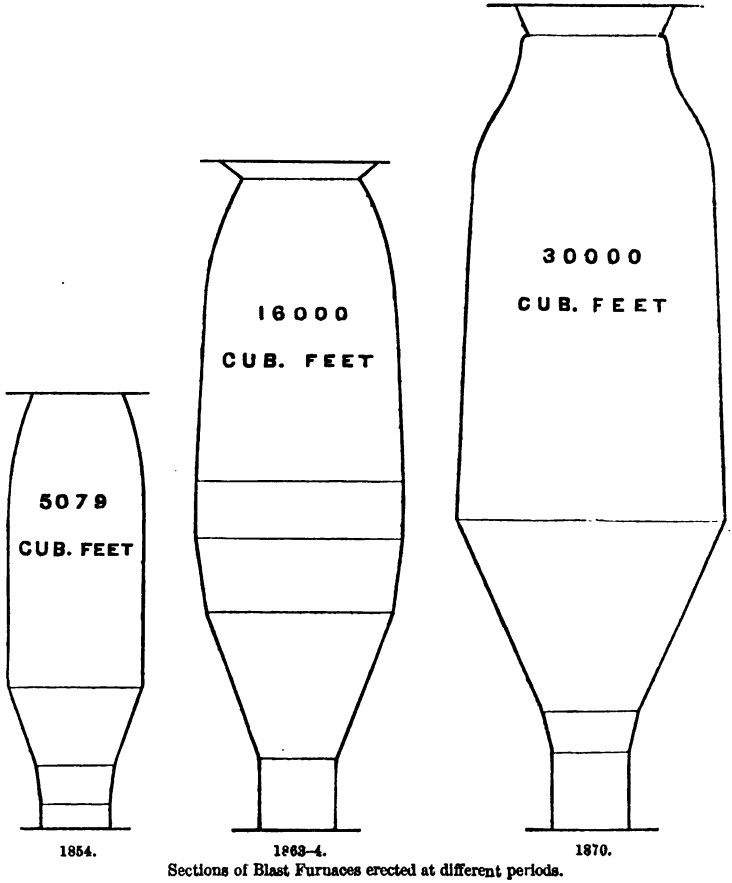
ore, the quality of the fuel and flux, the heat of the blast, and the capacity and regularity of working of the furnaces. In one of the furnaces (No. 6), about to be described, the average consumption of fuel, excluding the six weeks immediately after blowing in, has been 20·35 cwt. per ton of iron produced; the minimum quantity used

Fig. 1.



Sections of Blast Furnaces erected at different periods.

in any one week 18·78 cwt. per ton of iron, and the maximum quantity 22·12 cwt. per ton of iron. The average quantity of calcined ironstone used has been 46·11 cwt. per ton of iron; the minimum quantity used in any one week 44·16 cwt. per ton of iron, and the maximum quantity 48·04 cwt. per ton of iron. The average quantity of limestone used has been 10·71 cwt.; the

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minimum quantity in any one week 10·35 cwt., and the maximum quantity 11·26 cwt. per ton of iron. The average weekly produce of pig iron was 430 tons, the maximum 466 tons.[1] These quantities of coke and flux are about 15 per cent. less than those of five other furnaces erected in 1863-4, in which the same materials are used; but the internal capacity of which, 16,000 cubic feet, as against 30,000 cubic feet, is less in the proportion of nearly 1 to 2 than those being described, the heat of the blast being nearly the same.

The three furnaces first erected by the Author at South Bank, in Cleveland, in 1854, which had a capacity of 5,079 cubic feet, consumed from 32 to 40 cwt. of coke, and from 14 to 15 cwt. of limestone to the ton of iron; the temperature of the blast at the tuyeres ranging from 650° to 800°, whilst the weekly production of grey iron varied from 120 to 160 tons only.

The quantity of coal used in the kilns, to be described, in calcining the ironstone and limestone producing a ton of iron, is 3·94 cwt. With a regular supply of ironstone 3·50 cwt. is sufficient.

Plate 16 gives a ground plan and a corresponding elevation of the works. The whole of the raw materials, which are received in trucks, of which the gross weight varies from 10 to 17 tons, enter at the east end of the works, and descend by gravity to a steam lift, which raises them to a gantry surmounting a series of calcining kilns and coke boxes. The trucks have moveable bottoms, and the ironstone, limestone, and coal, used in calcining, are discharged from them into the kilns in proper proportions. The coke is allowed to fall into the boxes or bunkers, having hoppers and slides at the bottom, on opening which the coke falls into barrows, ready for charging in the furnaces. The calcined ironstone and burnt limestone, when withdrawn from the kilns, are likewise raked into barrows. The trucks when emptied descend by the drop, and are run into sidings ready for removal from the works. The barrows are raised to the tops of the furnaces by the steam lift. The slag flows from the furnaces into slag boxes or bogies, and is removed by a locomotive engine. The pig iron is tapped from the furnaces four times in every twenty-four hours, and is loaded on trucks from the pig beds. These trucks leave the furnaces by a railway in the opposite direction to the one by which the materials enter, joining the main line of the North Eastern railway at the western side of the works, where there is also a private wharf on the River

[1] Since this Paper was prepared, the produce per furnace has been increased to from 490 to 500 tons per week.—B.S.

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Tees, forming part of the works, at which vessels of from 600 to 800 tons can take in their cargoes. All trucks, whether full or empty, enter at one end, and leave at the other, by which means the greatest economy of labour and time in working the traffic is secured.

The blowing engines, four in number, are placed in the engine-house at the east end. The series of eight boilers for supplying steam to those engines, to the steam cylinder for lifting the trucks, and to the small engine which works the hoist, are placed as shown. The blowing engines all communicate with the horizontal cold-blast tube, whence the air is distributed to the stoves, in which it is raised to a temperature of 1250,° sufficient to give a heat of 1100° at the tuyeres; it is then discharged into the hot air main, and thence through the tuyeres into the furnaces.

The furnaces are closed at their mouths by a bell and hopper (cup and cone), and the waste gas, instead of as formerly burning at the tunnel-head, is withdrawn by the vertical tubes to an underground brick culvert, from which branches run to the hot-air stoves and to the boilers.[1] The combustion of this gas heats the stoves and raises steam in the boilers, so that no coal is consumed for either of these purposes, except in cases of stoppage. The entire consumption of coal in twenty-six weeks, exclusive of that used in first lighting up and in calcining, has been 190 tons, or $7\frac{1}{3}$ tons per week.

The number of men employed about the two furnaces in twenty four hours, exclusive of mechanics for repairs and plate-layers but including enginemen for removing the slag, is seventy-seven—fifty-two in the daytime, and twenty-five at night; being one man for every 11 tons of materials of every kind, including slag, transported; or one man for every $1\frac{1}{10}$ ton of pig iron produced.

A weighing-machine is placed at the entrance of the works for weighing the raw materials as they arrive. The pig iron is weighed on leaving the works, along with that of the other furnaces, at a point which is not included in the plan.

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[1] Just before adding a fresh charge of materials the temperature of the gas is upwards of 600°, and descends nearly to 300° when the charge has been introduced. As, temperatures of gases taken at top of No. 6 furnace:—

11.0 a.m. 315°, charge just lowered.
11.15 „ 436°
11.30 „ 634°
11.40 „ 335°, charging furnace.

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The general arrangement of the works having now been shown, the Author proceeds to describe their construction in detail. Commencing at the point where the raw material enters the works, the first operation, as before stated, is to lift it to the level of the kiln-gantry, or stage. The average weight of a loaded truck is about 14 tons, and the trucks are lifted 40 feet. The hoist for effecting this operation is shown in Plate 17, Fig. 1. It consists of a steam cylinder A, supported on columns, furnished with a piston and piston-rod, to the lower end of which is attached the crosshead of the cage B. The loaded trucks are allowed to enter the cage by a gentle descent. The steam is then admitted to the underside of the piston, and the load is lifted by the upward pressure of the steam to the higher level. The diameter of the steam cylinder is 38 inches, and its length is of course equal to that of the lift. As will be noticed, the cylinder is built in sections and bolted together with flanges, each flange being faced with a rebate, so as to secure an accurate fit. There is no difficulty in boring out all the sections perfectly parallel. When the truck is removed at the top, the cage is made to descend again by exhausting the steam. This, however, is not allowed to take place directly into the air, but through the pipe C to the upper side of the piston. The latter is thus kept warm by being at all times filled with steam; any entrance of air being prevented by the valve D, which opens outwards. The actual exhaust takes place during the next upward stroke, when the steam on the upper side of the piston is pushed, along with any condensed water, through the valve D, after which it passes by the pipe E to the cooling reservoir, joining the waste tuyere-water in its way, so that the steam is entirely condensed. It will be perceived that this form of hoist precludes the possibility of economizing to any great extent by expansion. Its great simplicity, however, as well as its effectiveness and perfect ease in manipulation, are advantages which outweigh other considerations.

The truck of raw material having been thus lifted, and having discharged its load into the kilns or bunkers, as the case may be, is passed on to the other end of the gantry, and lowered again to the ground level by the drop shown in plan (Plate 16). This drop may be briefly described as a friction-break apparatus, supported on cast-iron columns. A platform, suspended by wire ropes which pass round pulleys, is keyed on to the same shaft as the break wheels. The pulleys are provided with counter-balance weights, which outweigh the platform, and thus tend to keep it always at the top. When an empty truck is pushed on to the platform the balance is

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destroyed, and the truck is allowed to descend gently, by means of the break handle.

All the raw materials having been lifted, as described, the ironstone, along with the limestone and small coal, is tipped into the kilns, and the coke into the boxes. These latter are constructed of wood, and are two in number; each capable of containing 250 tons of coke. The flooring of each box is about 6 feet from the ground, and is provided with four sliding doors, through which the coke is shot into barrows as it is required. Besides these coke boxes, there is also an additional box next to the lift for storing small coal for the use of the kilns. The flooring of this box is high enough to admit a railway wagon underneath, so that, should the daily supply of small coal fall short, the kilns may be replenished from this source with the least amount of labour.

The kilns are five in number, and a section of one of them is shown in Plate 17, Fig. 2. It consists of an outer cylindrical shell of wrought iron, varying in thickness from $\frac{3}{8}$ths of an inch at the bottom to $\frac{1}{4}$th of an inch at the top, and an inner lining of fire brick, 12 inches thick. The interior diameter of the kiln is 26 feet, and it is tapered inwards at the bottom. There is also a central cast-iron cone, with its apex upward; the object of which is to hold up the charge and at the same time guide it conveniently towards the openings, out of which it is ultimately drawn. Each kiln has eight of these openings ranged round its circumference; and they serve both for drawing the charge as it comes down calcined, and for admitting the air for combustion in calcining. The capacity of each kiln is 15,800 cubic feet, representing 630 tons of mixed materials. In these works, although not always the case elsewhere, the limestone is tipped into the kilns along with the ironstone, by which means it is thoroughly dried and semi-calcined.

The next process is to remove the materials to the furnaces. Each barrow, for this purpose, when filled, is passed over a small weighing machine, and is then wheeled on to the cage of the furnace hoist. The entire lift to the charging platform of the furnaces is 92 feet, and the construction of the hoist is shown in Plate 18, Fig. 3. In this case the moving power, instead of being below as usually arranged, is placed overhead, and consists of a double-cylinder engine, with valves worked by link-motions. The diameter of the cylinders is 8 inches, and the length of stroke 12 inches. On the crank-shaft are two pinions working into two wheels on an intermediate shaft. At

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the middle of the latter shaft is keyed a larger pinion, gearing into the main spur-wheel (A), which is 12 feet in diameter. Each side of this spur-wheel is flanked by a grooved pulley, carrying a steel rope $1\frac{1}{4}$ inch in diameter. The ropes are made to fit the grooves with exactness, and they only pass half round their respective pulleys, the ends being attached in pairs to the two cages B B. Thus, while one cage is going up, the other is going down; and the work is done entirely by the friction of the ropes in their grooves. In order to secure equal tension on both ropes, the attachment to the cage is by a double lever, similar to a balance beam, which immediately yields to any unequal stretching of the ropes. The cages are steadied in their ascent and descent by guides attached to the columns C, which support the platform. The average weight raised at each lift is about 2 tons, although much more is capable of being lifted without the ropes slipping. It will be perceived that the moment the descending cage reaches the bottom, the strain on the ropes is relieved, so that they will no longer hold sufficiently in the grooves to enable the ascending cage to rise any higher. It follows that overwinding cannot possibly take place. The length of steam-pipe required for working the engine at this elevation is found unobjectionable in practice. The entire length of pipe from the boilers is 200 feet, and it is well covered with non-conducting material. The engines are usually worked at about 150 strokes per minute; and calculating for loading and unloading the cages, they are capable of making one lift per minute; and of thus raising in the hour 120 tons of material. The Author believes this lift to be the most economical, both in first cost and power, that has yet been constructed.

Plate 17, Fig. 4, is a vertical section of one furnace. The foundation up to the ground level consists entirely of brickwork resting upon clay. From this point a circular base is carried to a height of 7 feet in solid brickwork, mainly of fire brick, with a stone curb all round, on which the supporting columns rest. These columns are 18 feet 6 inches in height, averaging 2 feet 4 inches in diameter, with a thickness of metal of 2 inches. They serve to support the structure from the angle of the bosh upwards, the lower part being carried partly by the wrought-iron conical casing, and partly by the brickwork and staunchions which surround the hearth. The whole of the furnace from the tuyeres upwards is cased with wrought-iron plates, those of the lower or conical part being $\frac{1}{2}$ inch thick, while those of the barrel vary from $\frac{7}{16}$ths of an inch below to $\frac{3}{8}$ths of an inch at the top. The interior of the furnace is lined throughout with fire-brick lumps 5 inches

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thick, and of dimensions varying with the internal diameter, no two courses being alike. The backing between the inner lining and the shell is of ordinary firebrick. Up to a short distance above the tuyeres every fire lump is chisel-dressed on both beds and joints, and the same is also the case with the hearth lumps, which consist of two courses set on edge and breaking joint; the lower course being 18 inches deep and the upper one 3 feet. The following are the principal dimensions of the furnace: Diameter of the hearth 8 feet; depth at tuyere 3 feet 6 inches. Diameter at the bosh 28 feet. Diameter of the bell opening 13 feet. Total height from the hearth to the platform 85 feet. The cubical capacity is 30,085 cubic feet. There are four tuyeres, each with a muzzle 6 inches in diameter, and the dam opening is 2 feet wide. The pig beds are necessarily of large dimensions, being capable of holding twelve hundred moulds for each furnace. The slag boxes are eight in number, and large enough to contain upwards of 3 tons of slag each.

The construction of bell and hopper for charging the furnace is shown in Plate 17, Fig. 5. It consists of a bell suspended from a lever, having at its opposite end a counterbalance weight sufficient to keep it closed when not loaded with the charge of materials. The lever is connected to a piston in a small cylinder filled with water, having a passage by which the water can flow from one end of the cylinder to the other. So long as this passage remains closed the whole apparatus is immovable. Six barrow loads of material are tipped into the hopper, and rest upon the bell. A catch is now released, and opens the passage between the top and bottom of the cylinder. The lever is no longer held by the piston, which is able to move upwards in the cylinder. The weight of the bell and charge together overcome the counterweight; they descend, and the materials fall into the furnace. As soon as they are clear of the bell, the counterweight preponderates, the bell rises again, closing the furnace, whilst the piston descends and forces the water back through the passage from the bottom to the top of the cylinder. The passage is then closed, and the apparatus is ready for the next charge. This form of lowering apparatus is the invention of Mr. Wrightson, of the firm of Head, Wrightson, and Co., Stockton.

In each furnace the average consumption of air, of the density of the atmosphere, is about 8,000 cubic feet per minute, but it leaves the blast cylinders at a pressure of 4·5 lbs. per square inch above the atmosphere. This pressure is reduced to 4·25 lbs. by passing through the heating stoves, and finally to 3·75 lbs., by the

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time it reaches the tuyere most distant from the blowing cylinders.

The chemical reactions of the air and the charge in the furnace will not be described, as this has been done with remarkable ability by Mr. I. L. Bell, Assoc. Inst. C.E., Mr. C. Cochrane, and others.

The charge gradually descends to the zone of fusion, whilst the gaseous products of combustion, sufficiently charged with carbonic oxide[1] to be themselves combustible on coming into contact with air, ascend. The top of the furnace being closed, the gas escapes through an opening at the side and descends the wrought-iron main or down-comer, which is 6 feet 6 inches in diameter, into the underground brick culvert A (Plate 15, Fig. 4). This culvert traverses the entire length of the stoves and boilers, each of which is furnished with a branch flue and valve, so that each may obtain its share of gas according to its requirements.

The stoves for heating the blast consist of nine sections for each furnace, eight being always in action at one time, while one in rotation is allowed to cool for the purpose of cleaning. Plate 15, Fig. 4, shows the interior of one section. The gas is introduced by the valve B into the lower chamber, where it meets the air which enters by the openings C C. The gas and air being here thoroughly mixed, the flame arising from combustion plays among the cast-iron pipes, and issues by the chimney D. The blast enters the pipes from the cold-blast main E on the right, passing up and down until it reaches the hot-blast main F on the left. The latter is lined with brickwork 14 inches thick in order to prevent radiation, and from it the air is distributed to the various tuyeres by the shortest possible route, every precaution being taken to prevent loss of heat by surface cooling. The heating pipes are ∩-shaped in elevation, and oblong in cross section, as shown in plan. There are two rows in each section, six in a row, and they lean against each other at the crowns in order to force the flame to circulate between and at the back of them. The total internal heating surface of all the sections for one furnace is about 10,000 square feet. In addition to the gas valve each section is furnished with blast valves G G, both on the hot and cold sides, so that it can be entirely isolated from the rest at a moment's notice.

[1] The gas from the top of a furnace at Ferry Hill, 103 feet high, is stated in Mr. I. L. Bell's Paper on "The Development of Heat in Blast Furnaces" (Trans, Iron and Steel Institute, Sess. 1869), to be—carb. acid 14·30, carb. oxide 27·10, nitrogen 58·60.

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The cold blast main, as will be seen in the plan, Plate 16, takes its commencement at the back of the engine-house, there being a branch furnished with a valve from each blowing cylinder.

The blowing engines are of the vertical construction which has become so general in the Middlesborough district. They are four in number, but coupled in pairs, each pair having a fly-wheel between them with cranks at right angles (Plate 17, Figs. 6 and 7). The object of this arrangement is to give facility for carrying out the principle of expansion to a greater extent than is usually done. The resistance being direct and constant, there is in a single engine a difficulty in turning the centre under a high degree of expansion, and consequently of varying pressure, except with an unusually heavy fly-wheel. When a pair of engines is coupled, as in this case, the power throughout the stroke is equalised so as to admit of a higher degree of expansion. The steam is cut off by an additional eccentric working two plates on the back of the ordinary valve, and the grade of expansion can be altered, without stopping the engines, by turning the wheel at the top, which acts on right and left handed screws, so as to cause the plates to approach or recede from one another, and thus cut off the steam at any point desired. The diameter of the steam cylinders in these engines is 32 inches, and of the blowing cylinders 66 inches, with a stroke of 4 feet in both cases. The pressure of the steam at the gauge is 55 lbs., and of the blast 4½ lbs.; the speed 24 revolutions per minute. The steam is cut off after the piston has moved through a fourth of its stroke. Under these conditions the blast is sufficient for the production of about 950 tons of pig iron per week. The engines are sufficiently strong in all their parts to admit of a much greater speed of piston, and by working less expansively will develop power sufficient to blow an additional furnace.

The boilers for supplying the steam to these engines are eight in number, but there are never more than seven working at one time, every boiler being taken off for cleaning, in rotation, after it has been in work fourteen days. The diameter of the outer shell of each boiler is 5 feet 6 inches, and of the flue 2 feet 9 inches; its length is 35 feet. The boilers are suspended by means of saddles, which span the top. They do not derive any support from brickwork underneath. In front of each boiler is a combustion chamber made of fire-brick, and into this the gas is first admitted from the gas culvert by valves, the air being allowed to meet it by a concentric perforated pipe fitted with a regulator. The water for feeding the boilers is heated by the exhaust steam to about 200°, and the

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feed is effected by two donkey pumps, each having two plungers 4 inches in diameter; one donkey being sufficient to do the work in case the other requires repair. The burning gas on leaving the combustion chamber passes through a tubular flue and returns underneath the boiler to the front end, and thence by the smoke flue to the chimney.

The feed pumps are situated in the small building attached to the engine-house; and here also are placed the tuyere pumps, of which there are two, each with double plungers 10 inches in diameter. They draw water from the well, and pump it into the tank which forms the roof of the engine-house. From this tank the water distributes itself by gravitation to the various tuyeres, and then returns to the cooling reservoir, and from this again to the well, thus keeping up a constant circulation. The waste from evaporation and other causes is replenished from the water mains with which the town of Middlesbrough is supplied.

For working the traffic of these furnaces, and removing the slag, two locomotives are required. The former has cylinders 12 inches in diameter, with a stroke of 16 inches. The latter has cylinders 9 inches in diameter, and a stroke of 12 inches. As the traction is occasionally heavy, the wheels of the smaller engine are small (2 feet 6 inches in diameter), and the tread is short (5 feet), in order to enable it to turn round curves of small radius. Locomotives employed for this purpose, not requiring great speed, have been usually geared after the manner of traction engines; but the new construction is found to do better, and without the same liability to get out of order.

The following is a summary of the cost of the two furnaces complete—the details are given in the Appendix. The cost of railways would have been greater by about £2,500 if the ways laid down previously for five other furnaces had not been partially available for these; the time occupied in construction was fifteen months. The account does not include interest on capital during that time, and there is no charge for land:—

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SUMMARY of the COST of TWO BLAST FURNACES and PLANT, complete, erected at the Newport Iron Works, Middlesborough-on-Tees, in 1870.[1]

	£.	s.	d.
Two blast furnaces	10,517	9	2
Furnace gallery	857	14	10
Furnace hoist	973	9	11
Hoist engine and house	1,082	8	8
Kiln gantry	2,351	15	6
Kiln drop	794	5	3
Bunkers	1,885	16	4
Kiln lift	2,234	17	4
Five calcining kilns	4,327	18	4
Eighteen hot-blast stoves	6,241	1	8
Eight boilers and fittings	5,132	3	9
Two pairs of blowing engines	4,730	8	4
Engine house and tank	2,469	3	10
Cold-blast main	579	16	10
Hot-blast and horse-shoe mains	1,198	12	1
Gas down-comer and flues	1,723	10	8
Chimney	499	8	10
Force pumps, steam and water pipes, &c.	1,991	0	6
Floor plates and paving	580	11	0
Two locomotives	1,750	0	0
Eighteen metal bogies	522	0	0
Thirty slag bogies	733	1	7
Twenty charging barrows	90	0	0
One platform weighing machine	30	0	0
Railways, "equal to about 1½ mile of single line"	3,034	9	11
Total	£56,331	4	4

These furnaces were put into blast on the 6th of June, 1870, and up to the present time, with the exception of the usual cutting at the hearth, have worked satisfactorily. In spite of the high

[1] The fire bricks for the hearths and boshes were supplied by William Stephenson and Son, Throckley Brick Works, Newcastle.

The fire bricks for the remaining portion of the work, by Grieveson and Whitwell, of Newfield Brick Works, near Bishop Auckland.

The brickwork was executed by Potter and Wilson, Middlesborough.

The cup and cone arrangements by Head, Wrightson, and Co., Stockton.

The heating stove pipes were cast by Smith and Thomson, Stockton.

The hot-air valves were supplied by Cochrane, Grove, and Co., Middlesborough.

The furnace shells, tubing, &c., were made and erected at the Author's own works.

The blowing and lift engines were supplied and erected by John Stevenson, Preston.

The gantry lift cylinder and slagging engine, by Martin Samuelson, Hull.

The boilers and miscellaneous castings by Cochrane, Grove, & Co., Middlesborough.

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temperature, there is no appearance of wear in the heating stove pipes, which are constantly kept at a dull red heat.

During the last Session of the Institution a Paper was read by Mr. E. A. Cowper, M. Inst. C.E., on the Siemens-Cowper hot-blast stove.[1] That of Mr. Whitwell, differing in detail, but not in principle, has been described by him to the Iron and Steel Institute; and the whole subject of the effect of temperature of blast and of furnace capacity on the economy of fuel, depending upon the reactions of the constituents of the ores and flux, on the proportions of carbonic oxide and carbonic acid ultimately remaining over in the gases given off at the furnace-top, and on the temperature of these latter, has been already discussed before the Institution of Mechanical Engineers, the Iron and Steel Institute, and the Chemical Society, by Mr. Isaac Lowthian Bell and Mr. Charles Cochrane.

The question, in what precise degree, and within what limits, increased heat of blast and additional capacity of furnace tend to economize fuel has been variously answered by different observers, and is being tested practically at several works. Mr. Charles Cochrane has lately erected a still larger furnace, the blast of which is heated in Mr. Cowper's stoves to a higher temperature than that which the Author employs. If in using the ordinary Cleveland ironstone and fair blast-furnace coke he has succeeded in producing grey foundry iron more economically than in the furnaces described, it is fair to assume that he will not rest contented until he has ascertained what portion of that economy is due to temperature, and what to capacity; nor will it be long before his neighbours will try to attain similar results.

Meanwhile the works here described may serve as an example of a plant fully equal to the standard of the present day; and in which no pains have been spared to combine the two great essentials in the construction of iron-works, durability and simplicity.

The whole of the work, subject to the Author's general direction and approval, was designed and its execution was superintended by Mr. Richard Howson, the Resident Engineer of the works.

The communication is accompanied by a series of diagrams and drawings, from which Plates 16 and 17 have been compiled.

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[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxx., p. 309.

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APPENDIX.

DETAILS of the COST of TWO BLAST FURNACES, with the necessary PLANT, complete, erected at the NEWPORT IRON WORKS, MIDDLESBROUGH.

——	Number.	Tns. cwt.	Rate.	——	——
BLAST FURNACES.			£. s. d.	£. s. d.	£. s. d.
Excavating and concreting	..	..	..	21 12 1	
Red bricks for furnace foundation	233,500	..	1 2 3	260 18 3	
Ditto for pig bed walls	62,000	..	1 2 3	69 1 11	
Fire bricks for foundation and backing up	60,000	..	1 18 0	114 9 9	
Fire lumps for hearths and boshes	..	585 0	1 1 9	635 8 8	
Ditto for lining	..	642 0	0 16 6	527 10 6	
Stone curb and setting	..	..	..	127 4 0	
Lime and sand for pig bed walls	..	..	..	13 12 4	
Ditto for furnace foundation, &c.	..	..	..	52 19 0	
Fire clay	..	100 0	0 12 5	61 12 0	
Coals for portable engine, hauling lumps, &c.	..	..	..	34 12 6	
Brickwork for pig bed walls	..	..	..	37 14 1	
Ditto for furnaces	..	..	..	693 6 3	
Wrought-iron plates, rivets, &c., for 1 shell and tunnel head	..	103 5½	7 19 5	821 8 6	
Boiler-smith's work, making and erecting	..	..	..	322 2 8	
Bell and hopper	..	..	..	290 0 0	
12 columns for supporting upper lining	..	56 8	4 12 6	260 17 6	
4 sets belly pipes	..	..	..	54 1 6	
4 double slag boxes	..	41 17½	5 5 0	219 18 5	
4 water boshes	..	2 8	..	10 16 7	
Sundry castings, viz., buckstaffs, lintels, brackets, radius pipes, dam plates, tymps, slag troughs, &c.	..	73 14½	4 2 6	306 15 5	
Brass fittings, wrought-iron pipes, &c., tuyere water pipes	..	..	..	35 17 8	
Metal coping for pig bed walls.	..	9 0	3 0 0	27 0 0	
Stores and tools	..	..	..	62 3 3	
Wages	..	..	..	197 11 9	
Cost of one blast furnace	..	..	..	5,258 14 7	
Cost of two blast furnaces	..	..	..	.. ..	10,517 9 2
FURNACE GALLERY.					
Wrought-iron plates	..	40 15	8 15 0	359 0 2	
Boiler-smiths' work	..	..	..	268 2 11	
Metal floor plates.	..	48 0	3 0 0	144 0 0	
Wages	..	..	..	65 17 3	
Stores and tools	..	..	..	20 14 6	
					857 14 10

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Cost of Two Blast Furnaces, erected at the Newport Iron Works, &c.—*continued.*

——	Number.	Tns. cwt.	Rate.	——	——
			£. s. d.	£. s. d.	£. s. d.
Furnace Hoist.					
Red bricks	27,000	..	1 2 3	30 0 9	
Lime and sand	..	..	..	5 18 9	
Stonework	..	..	..	17 13 0	
Brickwork	..	..	..	16 17 3	
Columns, bases, and guides	..	57 6	8 2 1	462 0 8	
2 cages	..	..	..	145 7 9	
Wire rope	..	..	..	34 18 9	
Signal bell	..	..	..	0 18 0	
Wages	..	..	..	197 11 9	
Stores and tools	..	..	..	62 3 3	
					973 9 11
Hoist Engine and House.					
1 hoist engine	..	..	..	273 5 0	
Pulley and gearing	..	..	..	155 15 11	
Wrought-iron girders for pulley supports	..	7 3	15 5 9	107 5 0	
Cast-iron stanchions	..	5 14	6 8 0	36 8 7	
Small tank, copper pipe, lubricator, &c.	..	..	..	15 4 10	
Timber	..	..	..	105 10 11	
Plumbing and glazing	..	..	..	24 15 10	
Wages	..	..	..	301 19 4	
Stores and tools	..	..	..	62 3 3	
					1,082 8 8
Kiln Gantry.					
Wrought-iron plates	..	110 18	9 1 5	1,007 1 4	
Metal floor ditto	..	103 0	3 0 0	309 0 0	
Boiler-smiths' work	..	..	..	689 7 6	
Wages	..	..	..	263 9 0	
Stores and tools	..	..	..	82 17 8	
					2,351 15 6
Kiln Drop.					
Excavating	..	..	..	3 1 3	
Slag walling and brickwork	..	..	..	14 11 0	
Red bricks	4,300	..	2 2 3	4 16 3	
Lime and sand	..	..	..	9 14 3	
Columns, girders, balance weights, &c.	..	74 14	9 0 0	681 2 8	
Wrought-iron shaft and plummer block	..	1 1	..	21 8 4	
Ditto brake straps, &c.	..	0 18	..	18 0 0	
Wire rope	..	..	..	33 2 6	
Wrought-iron ladder	..	0 6½	1 6 0	8 9 0	
					794 5 3
Bunkers.					
Excavating	..	..	..	24 4 10	
Slag walling	..	..	..	228 10 4	
Red bricks	403,600	..	1 2 3	448 19 3	

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COST of TWO BLAST FURNACES, erected at the NEWPORT IRON WORKS, &c.—*continued.*

——	Number.	Tns. cwt.	Rate.	——	——
			£. s. d.	£. s. d.	£. s. d.
BUNKERS—*continued.*					
Lime and sand	..	..	..	226 8 7	
Stonework	..	..	..	33 1 9	
Brickwork	..	..	..	243 3 9	
3 wood bunkers with slides complete	..	..	..	594 16 2	
Wages	..	..	..	65 17 3	
Stores and tools	..	..	..	20 14 5	
					1,885 16 4
KILN LIFT.					
Excavating	..	..	..	72 4 9	
Slag walling and concreting	..	..	..	172 7 0	
Red bricks	13,700	..	1 2 3	15 4 9	
Lime and sand	..	..	..	108 1 10	
Columns and girders	..	75 8	9 0 0	678 7 2	
Pulleys and balance weights	..	..	..	48 10 10	
Cage	..	..	..	100 0 0	
Cylinder and piston rod	..	..	..	308 9 7	
Wrought-iron plates for casing	..	8 10	9 8 3	80 2 8	
Boiler-smiths' work	..	..	..	110 17 9	
2 lubricators	..	..	..	12 12 0	
Wrought-iron ladder	..	0 6½	1 6 0	8 9 0	
Wages	..	..	..	395 3 6	
Stores and tools	..	..	..	124 6 6	
					2,234 17 4
CALCINING KILNS.					
Excavating	..	..	..	4 17 7	
Slag walling	..	..	..	40 7 6	
Red bricks	47,000	..	1 2 3	51 18 1	
Fire lumps for lining	..	127 0	0 15 6	98 8 6	
Fire clay	..	..	..	4 17 8	
Lime and sand	..	..	..	34 13 7	
Brickwork	..	..	..	47 12 7	
Cast-iron feet, segments, lintels and plates	..	59 19	4 4 4	253 13 8	
Ditto cone	..	5 15	8 10 0	49 15 1	
Wrought-iron plates	..	23 2	7 12 2	175 15 9	
Boiler-smiths' work	..	..	..	68 19 0	
Wages	..	..	..	26 6 11	
Stores and tools	..	..	..	8 5 9	
Cost of one calcining kiln	..	..	..	865 11 8	
Cost of five calcining kilns	..	..	..	.. ..	4,327 18 4
HOT BLAST STOVES, EIGHTEEN IN NUMBER.					
Excavating	..	..	..	16 6 3	
Concreting and slag walling	..	..	..	68 4 0	
Red bricks	553,000	..	1 2 3	617 11 6	
Fire bricks	236,000	..	1 18 0	449 5 9	

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Cost of Two Blast Furnaces, erected at the Newport Iron Works, &c.—*continued.*

——	Number.	Tns. cwt.	Rate.	——	——
			£. s. d.	£. s. d.	£. s. d.
Hot Blast Stoves—*continued.*					
Lime and sand	..	..	..	163 15 10	
Fire clay	..	..	..	34 0 8	
Stonework	..	..	..	7 8 0	
Brickwork	..	..	..	538 6 4	
Metal borings	..	..	..	20 5 10	
Stove pipes	..	627 12	4 12 8	2,906 5 10	
Erecting pipes	..	..	..	151 0 0	
18 valves	..	..	..	25 4 0	
36 ditto 12-inch	..	..	5 10 0	198 0 0	
18 ditto 18-inch	..	..	8 0 0	144 0 0	
18 wrought-iron chimneys, dampers, and seats complete	..	..	..	90 13 4	
2 ditto ladders	..	0 4½	1 6 0	5 17 0	
Sundry castings, viz., connection-pipes, girders, fire-doors and frames, fire-bars, light-boxes, &c.	..	56 10½	5 0 10	285 7 5	
Wages	..	..	..	395 3 6	
Stores and tools	..	..	..	124 6 5	
					6,241 1 8
Boilers and Fittings, eight in number.					
Excavating	..	..	..	33 17 0	
Slag walling	..	..	..	66 4 9	
Red bricks	165,000	..	1 2 3	183 6 1	
Fire ditto	379,000	..	1 18 0	721 11 0	
Lime and sand	..	..	..	76 3 6	
Fire clay	..	..	..	54 14 3	
Brickwork	..	..	..	328 2 8	
8 boilers	..	..	..	1,856 0 0	
8 sets mountings	..	..	..	260 0 0	
8 Hopkinson's safety valves	..	..	..	75 12 0	
8 sludge valves	..	..	..	32 0 0	
Coating 8 boilers with Jones's cement	..	..	..	101 3 9	
Sundry castings, viz., saddles, fire-doors and frames, fire-bars, bearing-bars, flue-doors and frames, gas-inlets, &c.	..	75 13	6 7 2	477 12 1	
Wages	..	..	..	658 12 6	
Stores and tools	..	..	..	207 4 2	
					5,132 3 9
Blowing Engines, two pairs.					
2 blowing engines	..	..	..	3,800 0 0	
2 fly wheels	..	19 19	7 7 6	147 1 2	
1 valve, each 3-inch and 4-inch	..	..	..	30 18 8	
8 escape valves	..	..	..	20 0 0	
4 lubricators	..	..	..	19 0 0	
Oil drippers	..	..	..	20 15 2	
Wages	..	..	..	526 18 0	
Stores and tools	..	..	..	165 15 4	
					4,730 8 4

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Cost of Two Blast Furnaces, erected at the Newport Iron Works, &c.—*continued.*

——	Number.	Tns. cwt.	Rate.	——	——
			£. s. d.	£. s. d.	£. s. d.
Engine House and Tank.					
Excavating	..	..	..	33 12 8	
Slag walling	..	..	..	228 18 0	
Red bricks	406,000	..	1 2 3	452 7 6	
Floor tiles	..	..	..	18 2 6	
Lime and sand	..	..	..	232 18 11	
Stonework	..	..	..	14 14 8	
Slating donkey-engine house	..	..	..	4 11 8	
Plumbing and glazing	..	..	..	41 4 4	
Brickwork	..	..	..	254 13 7	
Joiners' work and timber foundations	..	..	..	225 19 7	
Wrought-iron plates for girders	..	4 9	7 11 1	34 7 5	
Boiler-smiths' work	..	..	..	13 5 6	
Perforated floor-plates	..	6 10	6 0 0	39 1 1	
Foundation plates	..	7 17	3 0 0	23 11 0	
Spiral staircase	..	..	..	30 7 8	
Wrought-iron ladder	..	0 3½	1 6 0	4 4 6	
Tank erected complete	..	80 10	8 0 0	643 19 11	
Wages	..	..	..	131 14 6	
Stores and tools	..	..	..	41 8 10	
					2,469 3 10
Cold Blast Main.					
Wrought-iron plates, rivets, &c.	..	33 4	7 17 7	259 19 9	
Boiler-smiths' work	..	..	..	180 11 8	
8 columns and saddles	..	4 15	8 0 0	38 0 0	
Relief valve and weights	..	..	..	6 13 9	
2 safety valves	..	..	..	8 0 0	
Wages	..	..	..	65 17 3	
Stores and tools	..	..	..	20 14 5	
					579 16 10
Hot Blast and Horse-Shoe Mains.					
Wrought-iron tubes	..	49 0	10 7 5	508 16 1	
Boiler-smiths' work	..	..	..	358 18 8	
20 cast-iron columns and saddles	..	8 8	7 1 2	60 14 2	
2 slide valves with racks	..	..	..	32 10 0	
Expansion rings	..	2 6	8 0 0	18 12 6	
Fire lumps for lining	..	110 0	0 16 6	90 15 0	
Brickwork	..	..	..	41 13 0	
Wages	..	..	..	65 18 3	
Stores and tools	..	..	..	20 14 5	
					1,198 12 1
Gas Down-Comer and Flues.					
Excavating	..	..	..	8 13 7	
Slag walling	..	..	..	16 14 0	
Lime and sand	..	..	..	47 0 0	
Fire clay	..	..	..	22 12 10	
Red bricks	168,000	..	1 2 3	189 0 6	
Fire ditto	144,000	..	1 18 0	273 6 9	

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Cost of Two Blast Furnaces, erected at the Newport Iron Works, &c.—*continued.*

——	Number.	Tns. cwt.	Rate.	——	——
			£. s. d.	£. s. d.	£. s. d.
Gas Down-Comer and Flues—*continued.*					
Brickwork	..	..	..	220 10 5	
20 escape valves	..	..	..	63 1 0	
2 throttle ditto	..	..	..	34 0 0	
2 girders, &c.	..	2 6	3 0 0	6 18 0	
Wrought-iron plates, rivets, &c.	..	42 15	8 5 9	354 8 8	
Boiler-smiths' work	..	..	..	314 1 7	
Wages	..	..	..	131 14 6	
Stores and tools	..	..	..	41 8 10	
					1,723 10 8
Chimney.					
Excavating	..	..	..	7 19 0	
Concreting	..	..	..	10 7 0	
Red bricks	234,000	..	1 2 3	262 18 3	
Fire ditto	5,000	..	1 19 0	9 18 0	
Lime and sand	..	..	..	57 19 10	
Brickwork	..	..	..	150 6 9	
					499 8 10
Force Pumps, Steam and Water Pipes, &c.					
2 4-inch double-ram steam-pumps for feed water	..	..	52 0 0	104 0 0	
2 8-inch pumps for tuyere water	..	..	130 0 0	260 0 0	
Valves for donkey-engine	..	..	..	50 9 6	
Suction pipes and clack-boxes for donkey-engine	..	..	..	15 11 5	
Super-heater	..	..	..	75 0 0	
Feed-heater	..	..	..	54 1 8	
Reservoir	..	..	..	150 0 0	
Steam and water-pipes	..	109 0	7 0 0	762 6 2	
Wages	..	..	..	395 3 6	
Stores and tools	..	..	..	124 8 3	
					1,991 0 6
Sundries.					
Fire lump paving	..	60 0	0 15 6	.. ..	46 10 0
Open sand floor plates	..	178 0	3 0 0	.. ..	534 1 0
1 locomotive for shunting	..	..	..	1,000 0 0	
1 ditto for slagging	..	..	..	750 0 0	
					1,750 0 0
18 metal bogies	..	..	29 0 0	522 0 0	
30 slag ditto	..	..	24 8 8	733 1 7	
					1,255 1 7
20 charging barrows	..	..	4 10 0	.. ..	90 0 0
1 weighing machine	..	..	..	.. ..	30 0 0
Railways, equal to about 1½ mile of single line	..	..	..	.. ..	3,034 9 11
					£56,331 4 4

[Mr. B. Samuelson

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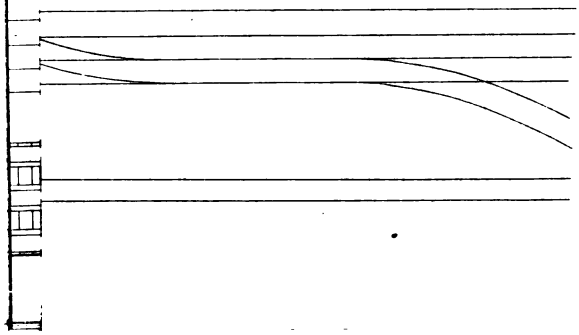

KELL, BROS LITHRS CASTLE ST HOLBORN

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[Mr. B. SAMUELSON

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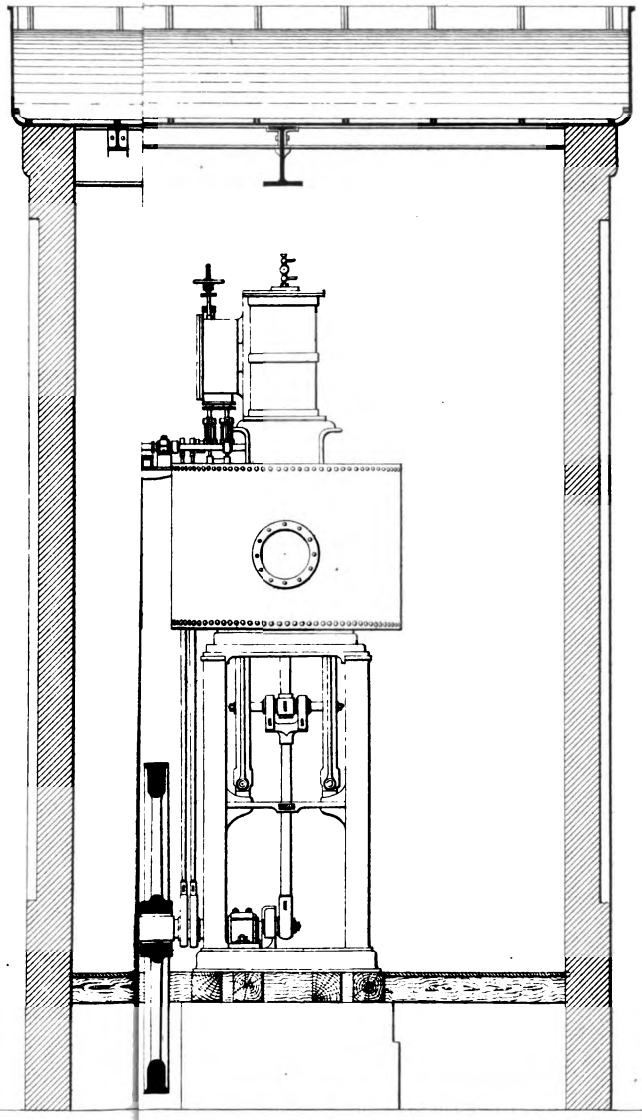
Fig: 7.



KELL. BROS LITHRS CASTLE ST HOLBORN

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Mr. B. SAMUELSON said he had endeavoured to describe the plant of a modern blast furnace rather from the point of view of an engineer than from that of a chemist. In the drawings (Plates 16 and 17) such details were given as, together with the dimensions and quantities stated in the Appendix, would enable any engineer to erect a blast-furnace plant.

He had stated the rapid steps by which the present large dimensions had been arrived at; and he doubted whether even now the utmost attainable perfection had been attained. But at any rate the Paper would afford a standard of comparison by which future progress might be judged.

Mr. I. L. BELL called in question some of the conclusions the Author had arrived at with regard to the operation of the blast furnace. In the first place, Mr. Samuelson spoke of hot air introduced into blast furnaces causing a very high temperature in the zone of fusion. He believed that was a fallacy; if the Author had been contented with saying, taking the quantity of fuel burnt, there was a greater quantity of heat evolved, in that opinion he might have agreed. There was undoubtedly a greater quantity of heat produced when the blast was heated, because that conveyed by the hot air was added to that produced by the combustion of the coke; but inasmuch as a greater amount of work to be done was imposed upon this extra heat by the larger weight of matter to be fused, practically, he imagined, the actual temperature of the hearth of a hot-blast furnace was reduced to that of furnaces blown with cold air.

The Author of the Paper said that the ore used contained 40 per cent. of iron in its calcined state. That being the case, he was at a loss to understand how it happened that, upon some of the occasions mentioned, the calcined ironstone yielded as much as 45 to 46 per cent. He was now smelting half a million tons of similar ironstone in a year, and he had never met with any which afforded above 1 or 2 per cent. beyond the percentage first stated, viz., 40. It appeared to him, taking 18 cwt. of coke per ton of iron, and ironstone yielding 46 per cent., that both were understated, and in about the same proportions; hence he conceived there would be danger in accepting 18 cwt. of coke as being able to produce 1 ton of foundry iron from the ordinary run of Cleveland stone. He felt he was on his trial in making such an assertion, but he would endeavour to explain the grounds upon which he questioned the accuracy of the Author's figures, so far as the power of any furnace to make 1 ton of foundry iron from Cleveland stone with 18 cwt. of coke was concerned, by examining shortly the operation of iron smelting.

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The Author had alluded to the introduction of air at the bottom of the blast furnace. This gave rise to the generation of an immense volume of carbonic oxide. The carbonic oxide of course was in an intensely heated state, and during its ascent in the furnace it first communicated heat to the descending materials, and then it served to remove the oxygen contained in the ore. If it were merely a question of depriving this ascending volume of carbonic oxide gas and its associated nitrogen of all their heat, by contact with substances of a lower temperature but inert in a chemical point of view, the subject for inquiry would present little difficulty; and similarly if the carbonic oxide could be saturated with oxygen before it left the furnace, it could then be determined whether the fuel had effected the maximum amount of duty it was capable of performing. But the question was a more complicated one, and he would give his reasons for thinking that it was impossible by any alteration of dimensions of the blast furnace to retain the escaping gases until their temperature was lowered to that of the atmosphere. The cold material in its descent in the furnace met a column of ascending heated gases. The heat generated by the action of the carbonic oxide on the peroxide of iron contained in the ore amounted to about 40 per cent. of the whole evolved in the furnace itself. This took place chiefly in the upper part of the furnace, so that practically in every blast furnace there were two fires; one at the lower part, where the carbon was burnt to its first stage of oxidation, and a second at the top, in which the carbon received its second dose of oxygen, and this determined the temperature of the escaping gases. Under these circumstances it was immaterial to what altitude the blast furnace were carried; a height of 100 feet or more would make no matter. That this was no mere speculation he had proved over and over again: he had taken the temperatures of blast furnaces varying in capacity from 12,000 to 35,000 cubic feet, and he had examined gases leaving furnaces of 40 feet up to 103 feet in height, and in no case beyond 80 feet had he found any marked reduction in their temperature.

A word or two with regard to the power of the carbonic oxide to take up a certain quantity of oxygen. The formation of carbonic oxide depended upon the union of an equivalent of oxygen with an equivalent of carbon; carbonic acid depended upon the union of the same quantity of carbon with two equivalents of oxygen. And, as he had said, if the carbonic oxide, in its work in the blast furnace, could be made to take up the second equivalent of oxygen, the total amount of heat to be derived from the union of these two substances would be obtained. But this was not so; and for the simple reason that the very gas which resulted from the reduction

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of the oxide of iron was of itself again capable of oxidizing iron. Conceive a vessel filled with carbonic oxide into which a small quantity of oxide of iron was introduced, and that it was heated to a certain point; almost the whole of that oxide of iron might be converted into spongy metallic iron. If now a higher temperature was applied the reverse of this action would take place. The newly-formed iron then commenced to rob the carbonic acid of a portion of the oxygen, and the iron passed again into a state of oxide, probably magnetic oxide of iron. In the blast furnace there were therefore two antagonistic powers at work. There was carbonic oxide first endeavouring to wrench from the oxide of iron its oxygen; and there was carbonic acid, resulting from that operation, endeavouring on its side to reoxidize the iron; and the question to be determined was, what was the condition of the relation of these two gases which constituted the neutral point? To that question he had addressed himself for the last two or three years, and his experiments justified him in believing that when to 100 volumes of carbonic oxide, 40 or 45 volumes of carbonic acid were added, the mixture at the temperature at which the gases left the furnace ceased to have any action on calcined Cleveland ironstone. The quantity of carbonic acid in the gases of a blast furnace was of course in direct proportion to the quantity of oxide of iron introduced; therefore the question to be determined was, what quantity of carbonic oxide must be present to preserve a reducing tendency and avoid one of an oxidizing nature. That quantity he had determined approximately in the laboratory, by an immense number of experiments, to be something like 40 or 45 volumes of carbonic acid to 100 volumes of carbonic oxide. Not satisfied with this experimental proof, he had recourse to a furnace emitting its gases with the carbonic oxide and carbonic acid in these proportions, and in them he exposed specimens of calcined Cleveland ironstone from periods of one hour to forty-eight hours. He apprehended that if there had been any actual reducing power still resident in those gases it would have made itself known upon ironstone exposed to their influence; but during forty-eight hours he never succeeded in expelling 6 per cent. of the original oxygen contained in the ore, a proof that practically these gases had been retained in contact with the iron ore long enough to exhaust their powers of reduction.

The Author stated that in a furnace with a capacity of 16,000 cubic feet he employed 15 per cent. more coke than he did in one of a capacity of 30,000 cubic feet. Without questioning Mr. Samuelson's figures, he could not accept this as generally true.

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The size of a furnace capable of doing perfect work was determined by the time required for a stream of carbonic oxide and its associated nitrogen traversing its contents to deliver to the materials as much heat as it could communicate, and to abstract as much oxygen from the calcined ore as they were capable of doing. He had satisfied himself that for this a furnace of 15,000 cubic feet sufficed; but he did not deny that the results obtained by Mr. Samuelson might be correct, although he believed the explanation to be that the lesser furnace of the two possibly had not been doing maximum duty. It might be said there was another way of looking at this question. What matter whether the furnace were large or small, provided the duty performed by the one was rateably the same as that of the other? As a matter of cost the ironmaster was served in the one case, so far as cost of plant was concerned, as well as in the other. This, however, was far from being the case, as might be seen on comparing the performance of Mr. Samuelson's furnace of 5,079 cubic feet, making 160 tons of iron per week, with one of 30,000 cubic feet, and running 500 tons of metal in the same time, *i.e.*, one furnace six times the size of another did only three times the work. The same held good when furnaces of larger dimensions were compared with each other, for he never found that a furnace of 25,000 cubic feet did twice the work as well as one of half the size. Making blast furnaces with so many thousand cubic feet more space would, without obtaining a corresponding increase of work, mean expending capital needlessly, *i.e.*, after sufficient capacity was reached to insure proper economy in fuel.

With regard to the question of superheated blast, until recently iron-smelters were satisfied if they heated the blast to a little above 600°. He believed the smelters in Cleveland were the first to point out, that the air could be very beneficially heated to more than that, and now the blast used was capable of fusing zinc, which was a temperature of about 780°, a change which had lessened appreciably the consumption of fuel. Encouraged by this success the same reasoning had been applied to the blast as to the blast furnaces. It was supposed that a rateable increase of economy would be derived from a further elevation of its temperature, just as it was considered that if fuel was saved by increasing the capacity of a furnace from 6,000 to 15,000 cubic feet, a further saving would result by adopting 20,000 and 40,000 cubic feet. He believed there was also a limit to the economy resulting from the use of superheated air; and notwithstanding the opinion of the Author of the Paper that perfection had not yet been reached, he believed

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that it had very nearly been obtained with the blast furnace and the mode of reduction in the present form. Looking at the properties of the Cleveland ironstone, he thought perfection had been as nearly as possible attained, because he found that after certain dimensions were reached, say 15,000 cubic feet, the escaping gases were as cool and as rich in carbonic acid as from furnaces twice this capacity in which longer contact had been maintained between the ore and the carbonic oxide. This language was equally applicable to the use of superheated air. Of 1,000 units of heat evolved in a blast furnace using air about 850° their derivation would be as follows: in round numbers about 480 of those units would be evolved by the conversion of carbon into carbonic oxide at the tuyere; 130 units would be contributed by the blast, and 390 of the 1,000 units would be derived from the conversion of the newly-generated carbonic oxide into carbonic acid. Now it was manifest that the expensive source of heat in these three items was that due to the combustion of coke; and it was therefore obvious that the more blast heat could be substituted for coke heat the better; but this substitution had its limit, because the reducing power of the gases must be preserved. A withdrawal of coke heat was equivalent to a withdrawal of carbonic oxide, and this was equivalent to a disturbance in the relation this gas bore to carbonic acid, which he maintained must be preserved. Mr. Charles Cochrane, however, had erected furnaces of 43,000 cubic feet capacity, which he intended blowing with air at a temperature of 1,400°, so that shortly there would be additional experience to bring forward. In the meantime he predicted that no great economy of fuel would result from these extraordinary dimensions or high temperature of blast. This assertion, based at first on theoretical grounds, had recently been confirmed at the Consett Ironworks, where two blast furnaces had been erected blown with superheated air. In one furnace of moderate dimensions, but perhaps too small for the amount of work it was performing, the results were fair with blast at about 1,200°; but even here the proportion of carbonic acid was below that due to perfect working. Hitherto, he might observe, all temperatures given by experimenters were probably too high; but recently Mr. Siemens had placed in their hands a pyrometer by which the temperature of the gases or blast could be more accurately observed, by ascertaining the amount of resistance caused by temperature to an electric current passing through a platinum wire; the previous errors, however, being common to all observations, were not very material. The Consett Iron Company, not satisfied with what they had accomplished, built

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a second furnace one-third larger, along with heating stoves capable of raising the temperature still higher than in that just mentioned, but the result was not what was expected, and appeared paradoxical to the manager, who stated that he was burning more coke, with blast of a higher temperature, at this furnace than at the other, and that the gases were leaving both equally cool, and he inquired what came of the heat? This paradox was explained by the laboratory, which proved that there was less heat generated in the second furnace than in the first, owing to there being less carbonic acid in the gases. He recommended a reduction in the temperature of the blast, and according to the last account this reduction of the temperature of the blast was being attended by some reduction in the quantity of coke required to produce a ton of iron.

The ASTRONOMER ROYAL remarked that the Paper gave the present stand-point in the history of the practical science of the reduction of iron ores; but he thought it might be interesting if he endeavoured to supply a more distant stand-point of the state of things at a former period. In the year 1824 he examined the various operations in the Staffordshire iron district; his head-quarters were at Birmingham, and he made excursions to Dudley and other places. He went into pits in the Staffordshire Main, and saw a good deal of the district generally. The simple results which he wished to communicate were these. At the time he was speaking of, it was accepted as a rule, established by experience, that to produce 1 ton of pig-iron 4 tons of coal were required, and to convert that quantity of pig-iron into malleable iron 4 tons of coal were also required. Those two numbers might be accepted as facts established at that time, and which might serve as a stand-point in the history of this practical science; also that the blast furnaces such as were used in those days turned out about 50 tons of iron per week.

The next opportunity he had of visiting the iron districts was in the year 1837, when he went into South Wales and saw what was done at the Merthyr and the Dowlais Works. This was then the general condition of things. The iron was for the most part puddled and reduced to a malleable state. The consumption of coal at Dowlais was considerably over 1,000 tons per day, or about 9,000 tons per week. Of that he was assured by Sir John Guest. There were then in blast at Dowlais, he was pretty sure, eighteen furnaces. The blast furnaces were scarcely bigger than those in 1824, and the hot blast was not introduced to any great extent. It was known at that time, for he recollected that there

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were some questions at issue about patent rights, but he did not see it in operation at Merthyr or Dowlais, so that the quantity of coal he had stated might be taken as representing the amount of fuel consumed in working these furnaces with the cold blast, viz. about 500 tons per week for each.

Making all allowances for the amount of coal consumed in refining and puddling, which applied to the greater part of that iron, and also for steam power for rolling, it would seem that the consumption was not much, if at all, reduced; and it appeared to him that the gain which had been made in regard to the saving of coal and the increased yield of the furnaces had taken place almost entirely since that time. His impression was, too, that no great improvement in the yield of the furnaces had been made between 1824 and 1837.

It was considered a point of some importance, introduced by Sir J. Guest, that after the operation of smelting he did not run the iron into pigs, but that he ran it in a hot state into the refinery, and in that way saved one heating. The causes of the great improvements that had taken place should be separated as far as possible, and therefore he was glad to see on one of the tables accompanying the Paper a clear account of how much was due to the temperature of the hot blast; but how far the general improvement might be due to other causes he could not say. Further information on this point was desirable. Both the size and the form of the furnace required careful consideration. Furnaces at the present time were less conical and more cylindrical than formerly. He would only add that it would be matter of great value if a competent ironmaster would undertake the history of the operations of refining and puddling. He was not himself aware what change had been made in them till the Bessemer period; a good account of those operations would complete the history of the manufacture of iron in England.

Mr. Bramwell observed, that he understood Mr. Bell to make what seemed to him the extraordinary statement, that the increase of the temperature of the blast did not increase the intensity of the heat in the zone of fusion, though it might increase the quantity of heat. Now, as a test, compare Mr. Bell's views with the theory and facts of the Siemens regenerative furnace, where the incoming gases and air were heated by the outgoing gases, so that each time they took up a higher temperature, and developed their heat upon the higher platform. In these furnaces, undoubtedly, the heating of the air and of the gas added to the intensity of the heat produced by combustion; and it seemed to him that this must be

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equally true in the case of the heated blast of blast furnaces, and that the theory of Mr. Bell must be incorrect.

Again, he understood Mr. Bell to say that the gases could not be removed from the blast furnace at a low temperature, because the carbonic oxide in deoxidizing the iron ore became carbonic acid, and that would evolve the heat due to the difference between the heat of combustion of carbon into carbonic oxide and into carbonic acid; and as that deoxidization took place in the upper part of the furnace, there must inevitably be an increase of temperature in the gases just as they were about to leave the furnace. He doubted the correctness of this view in respect to the development of heat by the deoxidizing of the iron ore. If carbonic oxide were changed into carbonic acid by combination with the oxygen of the atmosphere, or with other oxygen in a gaseous state, then heat would be no doubt evolved; but if the change from carbonic oxide to carbonic acid were made by combination with oxygen, which had to be obtained by the resolution of another combination in which it existed in a solid state, as in the case of the ore of iron where the oxygen existed in combination with the iron; then he thought the result of the process would not produce heat, as that evolved by the conversion of the carbonic oxide into carbonic acid would not be of sufficient intensity to deoxidize the iron ore.

Although he did not feel warranted in supporting this view from his chemical knowledge, he thought he might do so from observation of some experiments, or rather actual workings, in iron which he had investigated several years ago. It was probably twenty years since the well-known French chemist Chenot devised a means of producing steel by a process which he called 'cémentation à froid,' of which he would give a brief description, as perhaps the process was not generally known. In this process pure ore of iron—as hematite—was put into an air-tight chamber of the nature of a kiln, along with charcoal, or other matter containing pure carbon, and exposed to the action of external heat. Under the influence of the heat the carbonaceous matter united with the oxygen of the ore, and reduced the ore to what was called sponge of iron—a material which could be cut with a knife, being in fact iron in minute particles, held together by its mixture with earthy matter. This sponge of iron was then passed under edge-runners, or other mechanical appliances, and was crushed into powder. Part of this powder was mixed with resin, or fish-oil, or some other pure source of carbon. There were then mingled together part of the plain crushed ore and part of that which had been mixed

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with the resin or fish-oil, the proportion of the latter being greater as the steel was required to be more hard. The preparation was next compressed in moulds of the size of a bottle-cork, and the compressed material was put into an ordinary steel smelting crucible. Some of the very best steel he ever saw was thus made. He brought this instance forward to show that when carbonic oxide was converted into carbonic acid by union with oxygen previously in a solid state, it had not the power of deoxidizing the iron ore, and producing a balance of heat in the operation, although carbonic acid was formed; but that the application of external heat was required to enable the operation to commence, and that after that operation was begun there was not sufficient heat evolved to keep it going without the continuance of external heat. This indicated that the conversion of the oxygen from the solid condition, in which it was when combined with the iron, into a gaseous condition, required such an amount of heat, that all which was produced by carbonic oxide combustion was absorbed by the deoxidizing operation.

With these remarks, he would now pass to a non-chemical reason why it was impossible, by infinitely increasing the size of blast furnaces, to infinitely decrease the heat of the outgoing gases, and with this decrease to lessen the consumption of fuel.

The question to be considered was, whether by continuing to increase the height of the blast furnace the outgoing gases could not be caused to give up more of their heat to the incoming materials, and thus to economize fuel. It was argued, by those who advocated with this view the further heightening of furnaces, that as benefit had already resulted from the furnaces being made taller than before, there might be still greater advantage from continuing to increase the height. He thought those who thus argued overlooked the fact, that for one body to give up its excess of heat to another there was required not only a sufficient time during which the two bodies should be in contact, but also a sufficient capacity in the heat-receiving body to absorb the excess of heat that the other body had to give out. Now in blast furnaces, as originally constructed, there was not sufficient time for the transmission of the heat; but by heightening the furnace the time had been increased, and the gases had parted with more of their heat, and economy had been effected. But it did not follow of necessity that an extension of time must enable the gases to part with a further portion of their heat; for, as he before said, that would depend upon whether or not the capacity for receiving heat in the material which was introduced, in a given time, was sufficient

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to absorb the heat stored in the gases which went out in the same time. This seemed to him a very simple proposition; but in order to make himself clear, he had prepared some diagrams (Plate 18, Figs. 1—5), by the aid of which he hoped to make himself intelligible.

For the sake of illustration, he would assume that the problem was how to take out of a quantity of flowing hot water the whole of its heat above the temperature of a quantity of flowing cold water, and to transmit the heat to that cold water. This might be looked upon as a mere illustration, or as a case in actual practice, viz., where the continuous surface blow-off of a marine boiler was made to give up to the incoming feed-water the whole of its heat above the temperature of that water.

Let Fig. 1 represent a jacketed pipe, through the interior of which there should pass upwards in a minute 100 parts of water at a temperature of 200°, containing a total of 20,000 units of heat, and let there pass through the jacket round this pipe, and in a downward direction, in the same time, 100 parts of water at a temperature of 60°, containing 6,000 units of heat, and let the area and length of the pipe and the jacket be such that it would take two minutes for any particle of water to traverse the pipe and for any particle to traverse the jacket; and let it be assumed that the two minutes represented the exact time required for the transference of the heat. Then it would be seen that the complete transference would take place, the hot water would have parted with 14,000 units of heat to the cold water, so that the cold water would now have become as hot as the hot water was, and the hot water would have become as cold as the cold water was.

Next assume, as in Fig. 2, the same condition of things, with the exception that the pipe and the jacket being only half the length they were in Fig. 1, the time during which the two fluids would be passing through the apparatus would be one minute instead of two, such time could only suffice for the transference of 7,000 units of heat, and therefore the temperature of the outgoing water would only be cooled down to 130°, and the incoming water would only be raised to 130°. This would be an instance of want of effect due to want of time, as in the very short blast furnaces, and the defect could be cured by simply increasing the height of the apparatus to that shown in Fig. 1, as it was cured in a blast furnace by increasing its height, because there was sufficient quantity of cold water to take up the heat of the hot water if due time were given.

But assume, Fig. 3, that the quantity of incoming cold water

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be reduced so that 80 parts only came in during a minute instead of 100; the effect would still be the same as in Fig. 2 as regarded the outgoing water; viz., it would leave at a temperature of 130°, having parted with 7,000 units of heat; but the effect upon the incoming water would be different; for although it could only have absorbed the 7,000 units of heat, yet, being less in quantity than in Fig. 2, its temperature would be raised to 147½°, instead of to 130°, in the act of taking out with it 7,000 units of heat. Manifestly so long as the outgoing water was not raised to the same temperature as that of the incoming water, a better effect could be obtained by giving more time.

Let the apparatus be constructed as in Fig. 4, of such a height as to give one minute thirty-six seconds for the flow of the water: this time would be sufficient, at the rate of 7,000 units for one minute, for the transference of 11,200 units of heat; and 11,200 units of heat added to the 4,800 units representing the incoming 80 parts of water at 60° would raise those 80 parts to 200°, thus making them as hot as the incoming water; but that water would escape, after a deprivation of 11,200 units, with 8,800 units in it, representing 88°, or 28° more than that of the incoming water. A person observing this difference of 28°, and knowing that the difference of 70° in Fig. 3 had been lowered to the 28° of Fig. 4 by simply lengthening the apparatus, and thus increasing the time of contact, might imagine that he could get rid of the difference of 28° by a still further increase in the length of apparatus and corresponding time of contact, and he might therefore make it like Fig. 5, which was the same as Fig. 1, and gave two minutes of contact; but if he did this he would obtain no advantage whatever over the minute and thirty-six seconds of contact of Fig. 4, because that time of contact was sufficient to enable the 80 parts of incoming cold water to attain the same heat, viz., 200°, as the heat of the outgoing hot water; and as manifestly the cold water could never become hotter than the hot water that heated it, all increase in the length of the apparatus, so long as the incoming cold water was restricted to 80 parts in a minute, must be useless, and the only way of effecting a further reduction in the temperature of the outgoing hot water must be by increasing the number of parts of the incoming cold water, and if it were desired to obtain absolute uniformity of temperature in the outgoing hot and the incoming cold water, the quantity of the incoming water must be raised to at least 100 parts as in Fig. 1.

From the difficulty of making the proposition of the difference between time and quantity in this blast furnace problem clear,

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many persons interested in blast furnaces were directing their principal attention to increasing the height of those furnaces, knowing that such increase had been attended up to a certain point by the lowering of the temperature of the outgoing gases, and by a corresponding saving of fuel. But he believed they were being misled, because they did not bear in mind the fact, that the incoming materials in a given time remained a constant quantity in relation to that of the outgoing gases, so that when a height was attained sufficient to give time for the incoming material to take up from the outgoing gases the whole of the heat which that incoming material was competent to absorb, any increase in time must be entirely useless.

Mr. DAVID FORBES said that he was now, and had been since 1847, professionally and practically connected with iron smelting. He might also state, that in 1853 Mr. Samuelson consulted him before erecting his first blast furnaces in the Cleveland district. The Author had fully succeeded in describing and embodying in the Paper the main features of a first-class blast furnace plant of the present day. If the furnaces described in the Paper were compared with those which obtained ten years ago, it would be seen that independent of more perfect mechanical arrangements, the two main points of difference consisted in the tendency to augment the temperature of the blast, and in the tendency to increase the internal capacity of the furnace. On both these points he differed from Mr. Bell, inasmuch as he believed these were both steps in the right direction. He thought Mr. Bramwell had forgotten in his calculations the effect of the chemical action that took place in a blast furnace. It was not a simple case where air was sent in and out, but one where air was sent in, but compound gases sent out, owing to the chemical action brought into play; so that purely mechanical considerations were, to a great extent, out of the calculation. He believed, and he knew several eminent foreign metallurgists agreed with him, that the effect of superheated blast was not merely to increase the quantity of heat, but to increase the intensity of the heat at the focus of fusion. The object was not to increase the temperature of the upper part of the furnace, but to make it more intense at that part where the actual melting took place, where the greatest amount of heat was required to bring the whole into perfect fusion. For this reason he considered the limit to heating blast was merely the practical limit, which could only be proved by experiment, as to whether the blast could be more cheaply heated outside the furnace than inside.

The next point was as to the dimensions of the furnace. It had

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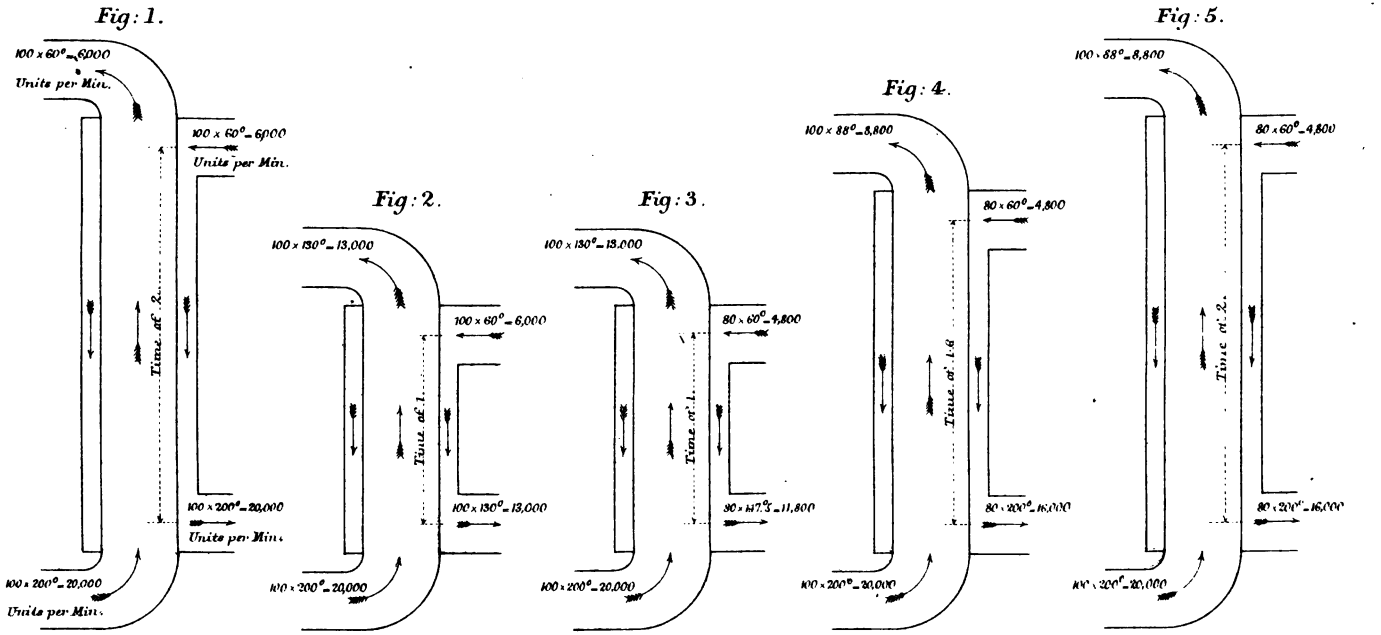
BLAST FURNACES.



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been stated as a proof that the proper dimensions for a furnace had been exceeded, that the larger furnaces did not produce the same increased yield of iron in the ratio of their increased capacity; that was to say, a furnace of 30,000 cubic feet internal capacity would not produce six times the amount of iron which a furnace with a capacity of 5,000 cubic feet would. Now he thought it would be quite as unreasonable to expect that, as to expect that the ironmaster who made 5*s.* per ton on his pig from small furnaces should make 30*s.* per ton on the yield from a furnace six times as large. Increased capacity was only one element in the calculation; but there were others which might altogether counteract its advantages, and which must be taken into account. The capacity of the furnaces of the present day was mainly increased in one direction only. Later experiments made in the Raschette furnaces of Russia and Germany had shown that there was reason to believe that an increased production of iron would follow the adoption of a very different form of furnace. It would be seen that the greater amount of the increased capacity of the furnaces described by the Author was in the shaft of the furnace, and not in the real working part—the focus of the furnace. He thought, therefore, that when the subject was studied more carefully, far better results would hereafter be got from the large furnaces than at present. It took many years, and great numbers of experiments, before the proper dimensions of the parts of an ordinary small furnace were obtained so as to get the conditions of most economical working; whereas, at present, there were but few examples of large furnaces, and these were made without any further guidance than what experience had taught in the small ones. A more intimate acquaintance with the larger furnaces would, he thought, lead to better results.

Mr. C. W. SIEMENS observed that the information contained in the Paper and elicited by the discussion went far to show that, by carrying out a chemical operation on a large scale, the utmost results which theory indicated could be almost realised. Two years ago, when the question of large furnaces was first mooted, he made a calculation of the work done in a blast furnace, and of the heat that was developed in that blast furnace. By taking into account, on the one hand, the ore to be heated and reduced, the pig to be melted, the slag to be melted, and the heat and composition of the escaping gases at the temperature at which they were carried away; and, on the other hand, the temperature of the blast and the heat due to its combustion with coke, in producing the gaseous mixture escaping from the top of the blast furnace, he found that 20 cwt. of ordinary coke should suffice

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theoretically to smelt a ton of pig iron from Cleveland ironstone containing 40 per cent. of metallic iron, and that 21 cwt. per ton of iron was a result that might be practically realised. Assuming the temperature of the blast at 1000°, the best results at that time were about 26 cwt., but it struck him that Mr. Samuelson had obtained the calculated minimum result almost completely; his consumption being 20½ cwt., and the temperature of blast about 1100°. Mr. Bell arrived at nearly the same figures by another mode of reasoning, and had followed up this subject very minutely, by examining into the particular stages of the chemical operation carried on in the blast furnace, deducing very valuable data for the metallurgist.

Nevertheless, he would not recommend others to follow Mr. Samuelson under all circumstances in the construction of a blast furnace. He was at that moment constructing some blast furnaces of very different dimensions; they would only have a cubical capacity of 10,000 feet instead of 30,000 feet; for he believed it would be inexpedient in this case to adopt such extreme dimensions. It would require the peculiar conditions of the Cleveland iron district in order to do so with advantage; namely, an excellent hard coke and an ironstone rather strong and comparatively poor. For richer ore and tender coke he believed the size of blast furnace such as Mr. Samuelson had described would be inapplicable.

With regard to the effect of extreme capacity, he agreed with Mr. Bramwell's views, while at the same time he admitted that Mr. Forbes' remarks were perfectly correct. But Mr. Bramwell spoke of the mechanical absorption of heat—the regenerative action within the blast furnace;—on the other hand Mr. Forbes spoke of the chemical action between the gases of the blast furnace and the ores; and both these actions were subject very much to the same laws. To get from the fuel a maximum of effect in a steam boiler plenty of heating surface was given, as in the case of the Cornish boilers; and it was reasonable to suppose that more surface would give superior results, simply because the difference of temperature between the heat-transmitting current and the heat-receiving surfaces was brought down to a minimum. Where a current of gas flamed rapidly through a mass of solid matter, there must be a considerable difference of temperature between each particle of the fluid and the solid receiving that heat: whereas, if the mass was greater, and the current relatively slower, there must be a more perfect adjustment between the two temperatures, giving rise to a more economical result. In the same way the

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chemical action would be more perfect if a gas containing a certain proportion of carbonic oxide (mixed with carbonic acid and nitrogen) had plenty of time to communicate its carbon to the piece of ore which was ready to take it in yielding up its oxygen. If the quantity of material present were infinite a perfect adjustment between these conditions would arise, depending upon the temperature and the proportional amount of carbonic oxide present; but such a condition of equilibrium could never be obtained. It was therefore necessary in practice to consider how much would suffice for the purpose, because other considerations entered into the subject. If a blast furnace was very large in proportion to the quantity of work that was to be performed in it, a greater economy would be attained by a relative increase of first cost. This was the case in Mr. Samuelson's blast furnaces, where 30,000 cubic feet capacity produced only 500 tons of pig metal per week; whereas a furnace with a capacity of 10,000 or 11,000 cubic feet would produce 300 tons. It was therefore clear that these large furnaces produced approximately only about one-half the amount of work per cubic foot of capacity that the smaller furnace did. The question arose how far was it judicious to expend more capital to obtain additional saving, if other means might be resorted to of arriving at the same, or even a greater increase of economy. This point had not been touched upon in the course of the discussion. Mr. Bramwell had shown that where there was a greater outgoing current than a heat-absorbing material to meet it, an unstable condition or waste of heat must ensue; and Mr. Forbes had argued in the same way on chemical grounds. In the blast furnace the object was to waste as little heat as possible, and to bring out the gases at the top as cool and as thoroughly deprived of their chemical or reducing power as was practicable. Both these objects were obtained by diminishing the relative quantity of gases. If, for instance, the relative amount of gases for a given amount of incoming material could be reduced by one-half, and if the chemical conditions were the same, it was clear that the smaller quantity would be more thoroughly deprived of its heat; but the smaller quantity would have had time to divest itself more thoroughly of its chemical action also: there would be a larger relative amount of carbonic acid gas produced, and the coke would be consumed in the furnace to greater advantage. This could be accomplished by a blast of high temperature; and in this respect his opinion differed very much from the theory of Mr. Bell, to the effect that there was a theoretical as well as a practical limit

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to the beneficial heat of hot blast. He believed, on the contrary, the blast could not be made too hot for economical purposes, and that real progress in iron smelting must henceforth be looked for chiefly in that direction.

Mr. E. WILLIAMS said he had been unable with blast furnaces as now constructed to obtain any appreciable advantage by increasing the heat of the blast above 900° or 1000°; and in this respect his practical experience confirmed the belief expressed by Mr. Bell. At a large ironworks in the North of England, it had been proved by experience that an excessive temperature of blast was prejudicial. At one of the furnaces of the Consett Iron Company the blast was heated to 1400° or 1500°, and there was a greater expenditure of fuel per ton of pig iron made than at an adjoining furnace where the heat was 1200°. This, he was convinced, was the case; though, personally, he had no experience of blast of a temperature much beyond 1000°.

The shape of the Middlesbrough furnaces had been objected to. On this point he would only say, that, if Mr. Forbes would give a sketch of a better furnace, the iron smelters of that district, who were always anxious to improve their furnaces, would be happy to give it a trial, provided it was not too costly, and it was thought there was a fair chance of success.

With regard to size of furnace, economy of fuel was due to increased capacity in so far as the height was increased, but widening the furnace would not produce economy of fuel, though it produced larger makes of iron, and, therefore, a larger factor by which to divide the general charges. No economy of coke resulted from increasing the capacity of a furnace in any direction other than in its height. He had under his management at Middlesbrough and elsewhere furnaces of different heights, with an internal capacity varying from 11,000 to 30,000 cubic feet, and he found that the taller furnaces, 95 feet high, used least coke; and of these, two furnaces with the bosh 16 feet in diameter used rather less per ton of pig iron made than furnaces of the same height with wider boshes. This difference, he thought, was explained by the fact that the narrow-bosh furnaces had a thicker lining of brickwork than the furnaces of larger internal dimensions. The height should be fixed so that the weight of the column of materials should be rather below the crushing point of the most friable material charged. For finely-divided ore, that was crushed already, the very tall furnaces would not answer; but where the furnace was charged with material in moderately-sized lumps, sufficiently large to allow the blast to pass up through the interstices (and

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when it ceased to do that the furnace would not work at all), the higher the furnace the better, so long as the crushing point was not reached. As a fact, the richer ores of this country were in a fine state of division, but it was the mechanical condition of the ore which made it unsuitable for tall furnaces, not the richness.

Mr. C. W. Siemens observed that tall furnaces were not suitable for rich iron ore; for if it was finely divided, such as hæmatite, the minuteness of the subdivision was prejudicial to the free operation of the blast; but if it was in large lumps the ore would be liable to fuse before it was reduced, and 'gag' the furnace.

Mr. John Lancaster, M.P., remarked that he was much in the same position as Mr. Williams, in so far as what little knowledge he had of the subject had been acquired by practical experience; he, too, frequently had to change his opinions as to certain results obtained. In a commercial point of view there was much to admire in the representative furnaces of 1871; but he thought it would be a mistake to consider them as representing the various districts of iron manufacture. They were, no doubt, adapted for particular ores and fuel, both of which were of a very special character. The fuel of the district in which these furnaces were situated was perhaps mechanically the hardest fuel in Great Britain, and hence would withstand greater pressure and a higher furnace than any other. In 1852 he erected some furnaces in North Staffordshire 60 feet high, the old standard being 48 feet, but that height was more than the mechanical structure of the fuel could endure, and he was obliged to mix it with imported fuel to support the height of the furnace. He would therefore caution iron smelters, in districts where the fuel was not mechanically strong, against resorting to such lofty furnaces as these. Under any circumstances he questioned whether, taking the cost of the structure into account and looking at the quantity of iron produced, it was wise to construct furnaces of such large dimensions. He had himself erected furnaces 80 feet high with a 23-feet bosh, but he had reverted to what he considered the more economical height of 65 feet for a mixture of hæmatite and other ores. He therefore thought that furnaces of half the capacity of those described in the Paper would yield a better commercial result upon the capital expended.

Mr. J. Kitson inquired whether the increase of temperature in the furnace had not a tendency to make the iron tender? He had himself a predilection for cold-blast iron.

Mr. I. Lowthian Bell said he still retained his opinions respecting the cause of a certain temperature being maintained in

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the escaping gases of a blast furnace; he also differed entirely from Mr. Siemens and Mr. Forbes when they said that there was no limit to the heat of the blast producing a saving of fuel in the smelting of iron. In answer to Mr. Kitson's inquiry, a record had been kept for some years at the Clarence Works of the breaking weights of the metal produced at that establishment. No appreciable difference had been observed since the blast of a higher temperature than formerly had been used, but what the effect might be when the air was raised to 1400° he was unable to say.

Mr. E. A. Cowper observed, on the subject of the temperature of the blast and the advantage that was found to result from raising it higher than usual, that he might state from an experience extending over more than twelve years, that it was found in raising the temperature every 100° so much was gained in the saving of coke, and, with Cleveland ironstone, the advantage continued with every rise in temperature up to the present, as it was natural to suppose it would, and a temperature of considerably upwards of 1400° had been maintained for months together. Good results had been obtained under very high temperatures of blast when hæmatite was mixed with the Cleveland ironstone. Iron-masters were becoming much more impressed with the saving that could be effected, by greatly increasing the temperature of the blast, and many both at home and abroad were adopting the regenerative high temperature hot-blast stove. With a furnace of 20,000 cubic feet capacity a saving of 4 cwt. of coke per ton of iron made had been obtained by simply increasing the temperature of the blast from 1000° to 1442°.[1] He had received the exact returns of the consumption of coke per ton of iron made at Consett, with different temperatures of blast, and found the saving was as follows:—No. IV. furnace, a saving of 1 cwt. 2 qrs. 27 lbs. of coke by raising the temperature of the blast from 1200° to 1400°, and with No. V. furnace a saving of 1 cwt. 3 qrs. 8 lbs. by raising the temperature of the blast from 1250° to 1500°. Mr. Samuelson had no doubt arrived, as nearly as possible, at the greatest economy of coke that could be obtained with the temperature of blast he used, and the materials he dealt with, but he had got it at the cost of 10,000 cubic feet of capacity in the furnace, the furnace having 30,000 cubic feet capacity in place of 20,000 cubic feet, which was generally considered large; and that, he apprehended, was obtained at a

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxx., p. 319.

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cost of probably £5,000, besides the additional working expenses consequent on the larger scale of the works. He would merely add, that a considerable saving could be obtained with a smaller outlay, if the temperature of the blast, instead of the size of the furnace, were increased.

Mr. B. SAMUELSON, in reply upon the discussion, said that Mr. Bell had stated his belief that the intensity of the heat in the zone of fusion was not augmented by increasing the heat of the blast; whereas Mr. Forbes and Mr. Siemens appeared to be of opinion that it was increased. Those gentlemen were able to form an opinion as chemists whether that was the case or not, and they differed; but he, not being a chemist, had no right to make any statement on the subject because he did not know, nor did he think that any practical iron smelter had ascertained the heat in the zone of fusion of a blast furnace. He must apologise for having stated, definitely as a fact, what he did not know. Mr. Bell seemed to think that he was mistaken as to the quantity of fuel used in these new furnaces, and that he had failed to compare a furnace of large capacity in its best state with a furnace of small capacity in its best working condition. Now he was satisfied he had made a fair comparison; but inasmuch as he might have been deceived, he had taken pains to verify the statement, and he found, from an examination of his books, that the statement of the saving effected was quite within the mark. It could only be possible that an error had occurred in this statement, either if somebody had charged for a large amount of coke that had not been received at the small furnaces, or that some one had made him a present of a large quantity of coke for the large ones which had not been paid for. In reference to the one contingency he had the fullest confidence in his agents; in reference to the other contingency he should be sorry to think so ill of the management of coke and coal owners of South Durham as to suppose that it could arise. The figures he had given had been carefully verified; they had been taken under fair and proper circumstances for comparison; and were the result, not of a week or a fortnight's working, but upon a statement of accounts of fuel received and paid for over successive periods of six months.

The next question was—What risk had he run by increasing the capacity of these blast furnaces very much beyond those which he had previously constructed? In two ways he did not run any great risk. He did not jump suddenly from a furnace with a capacity of 16,000 cubic feet to a furnace of 30,000 cubic feet, but he watched what had been going on in his neighbourhood; and he

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found, somewhat in contradiction of the experience of Mr. Bell, that economy had, in every case, been derived from increased capacity.

But there was another view in which he satisfied himself that he was not running any great risk; he referred to the opinion of Mr. Cowper, that increasing the capacity from 15,000 cubic feet to 30,000 cubic feet involved an additional cost of about £5,000 in the construction of the furnace. Now, the actual total cost of the large blast furnace *per se*, as shown in the abstract, was £5,000; consequently it was impossible there could be an increase of £5,000. No doubt there was an increase perhaps of £1,500 or £2,000; but there were savings in other ways. There was saving in the simplification of parts. If in one vessel 500 tons of iron were made instead of 300 tons of iron, there was a saving on the ton of iron in the tuyere arrangements and communications from the heating stoves and in other details; so that, in some measure, the increase of cost in the actual dimensions of furnace was partly made up in other ways.

He was now erecting a third furnace, as nearly as possible of the same dimensions, for less than one-third the cost for two, say for £14,000, so that for £70,000 he should have a plant of three furnaces; for he had already sufficient engine power, sufficient kiln power, and the other arrangements provided for two were sufficient for the three. He had observed this, and it had become a rule in the Cleveland district, that the cost of a good blast furnace plant, within the limits of proper economy, was £1 per ton per annum of the production. If the plant cost £40,000 the produce was about 40,000 tons of iron; if the produce was 70,000 tons of iron the expenditure would be £70,000; and this would be carried out in the present instance.

Apart from the chemical points raised by Mr. Bell, there were many mechanical considerations with reference to blast furnace plant which had been overlooked in the discussion. He had stated in the Paper, that with a pressure of $4\frac{1}{2}$ lbs. per square inch at the blowing cylinder he only obtained a pressure of blast of $3\frac{3}{4}$ lbs. at the tuyere. That was a bad result, and it was a point which he hoped would have been examined, so that a better result than he had stated might be obtained in future. There were allowances to be made for expansion and friction, but the loss of pressure was too great and ought to be diminished.

In reference to the percentage of iron contained in the ore, when describing the ironstone of Cleveland, he said that, in a calcined state, it contained from 37 to 40 per cent. of iron; and that state-

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ment was fair as a general description of the ironstone of the district; but he quite agreed with Mr. Bell that, on an average, less than 40 per cent. of iron was obtained. The minimum yield of between 18 and 19 cwt. of coke, which Mr. Bell considered impossible, applied to one week only, and might be attributable to the weighing up of some of the iron produced having been carried forward from one week to the next. This was, of course, corrected in the averages. As to these latter, Mr. Siemens was of opinion that he had arrived at a result which very nearly corresponded with the theoretical result; while Mr. Bell thought he had not obtained any such results, because being the best obtainable theoretical result he could not arrive at it in practice. But what he contended was, that nobody knew what was the most perfect theoretical result. The number of units of heat to produce a ton of iron had been estimated, it was true, in the laboratory; but he did not believe there was one chemist who had ascertained for himself approximately in the laboratory the theoretical quantity of heat required for all the operations of the blast furnace. Tables had been constructed combining all these as ascertained; some by one authority, others by another, and the sum of them had been taken as correct. He believed it would be a marvellous thing that the theoretical result should be correct; for he was not aware that in any other manufacturing or chemical process theoretical perfection had been attained within 15 or 25 per cent.; and he was fully persuaded that, in the present instance, there was a margin for a further saving.

That there was a further margin for saving he could prove in this way:—He had taken off the gases from the top of the large blast furnace at intervals of half an hour, the interval being the time between the introduction of one charge and the introduction of another. He found that between those two periods there was a difference of more than 300° in the temperature of the gases; for whereas immediately after a charge of cold materials had been introduced the temperature was about 300°, at the end of half an hour the temperature was upwards of 600°. He thought it was desirable to approximate more closely to an average temperature of the materials than this. There was no theoretical reason why there should be a rise of temperature of 300° between those two periods. If the volume of materials charged into the furnace in a given time bore a smaller ratio to the total quantity of material in the upper part of the furnace, the reduction of temperature by the introduction of the cold material would be smaller, or, what meant the same thing, the actual temperature, before the introduction of the material, would not be so high.

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Capacity as distinguished from mere increase of height had been referred to by Mr. Williams; but what followed an increase of height of furnace? Why the mass of materials which was to absorb the waste heat of the gases was also increased:—in fact, the regenerative principle of Mr. Siemens was carried out. But the same occurred if the diameter was increased; and in practice increase of height had this disadvantage—it had a tendency to crush the fuel which was charged into the furnace, and thereby to increase the difficulty of the passage of the air through it. These were distinct considerations—the one practical as to facility of smelting, the other as to economy of fuel.

He had only to add, in confirmation of what fell from Mr. Bramwell, that there was an element in the economy of fuel in the time allowed; that whilst it took between sixty and seventy hours for a charge to descend in the larger furnace, in the smaller one this took place in from thirty to forty hours.

May 9, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

THE discussion upon the Paper No. 1,299—"Description of Two Blast Furnaces, &c.," by Mr. B. SAMUELSON, was continued throughout the evening.

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May 16, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

No. 1,304.—"The Treatment of Town Sewage."[1] By ARTHUR JACOB, B.A., Assoc. Inst. C.E.

THE disposal of town sewage is one of the most important problems that has presented itself for solution in the whole course of sanitary research, and whilst the methods adopted for the removal of liquid refuse from human abodes have been successfully determined by practice, a very great divergence of opinion exists even now with regard to the mode of dealing with that refuse at the outfall.

Waste, on the most gigantic scale, has been permitted for ages, and is still allowed to continue, even at the very centres of civilization, and rivers continue to be polluted because the public cannot feel assured as to the safest and most economical treatment of their sewage.

In summarizing the various processes that have been suggested for the treatment of sewage, each will come under one of the following general heads:—

1st. Chemical.
2nd. Mechanical.
3rd. Agricultural.

It is proposed to review, briefly, the attempts made up to the present time to deal with sewage by each of the first two classes and then to enter more fully into the process of sewage irrigation, which so far appears to offer the most efficient solution of the question.

Before treating however of the Disposal of Sewage, it may be well to consider briefly the merits of what is called the midden system, not as having any close connection with the question of the treatment of sewage, but because endeavours have been, and are still being made, to offer the dry system to the public as a panacea for all the trouble and danger in which they find themselves

[1] The discussion upon this Paper occupied portions of two evenings.

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involved. The object of this system is to treat human excrement by dry earth, ashes, or some other absorbent material, so as to deodorize it, and deprive it of its offensive properties. No doubt dry and absorbent materials generally, and certain kinds of earth in particular, possess the property of destroying smell, and of arresting, to a great extent, the decomposition of animal refuse. This is probably only an illustration of the theory propounded by the celebrated chemist, M. Dumas, that chemical action does not take place at ordinary temperatures without the presence of water. But the admission, that excreta may be successfully treated by the dry process, will never lead to the conclusion, that this system will obviate the necessity for drainage.

An ordinary household of, say, six persons, enjoying a proper water-supply, will consume for all purposes about 20 gallons per head per diem, or a total daily quantity of 120 gallons, which must necessarily be fouled in its use, and pass away from the house laden with dangerous impurities. If earth closets are used instead of water-closets, a certain quantity of water, perhaps as much as 4 or 5 gallons per head, may be saved, but there will still be 15 gallons of water fouled with grease, soapsuds, vegetable refuse, and other materials of a highly putrescible kind, which must necessarily pass away and be disposed of at the outfall. Under special circumstances, and in certain situations, the earth system is doubtless found applicable; but it is a fallacy to offer it as a substitute for sewerage, and therefore, as it does not remove the difficulty or reduce the amount of sewage to be dealt with in any appreciable degree, the consideration of it may be for the present dismissed.

Chemical Treatment.

Under this heading it is proposed to consider only those methods which aim at the reduction of the evil by some specific chemical treatment, that will either decompose, or render more stable, the offensive constituents of the sewage. Many processes have been devised with one or other of these objects in view; but all have failed to accomplish the desired end. In nearly every case the several modes of treating sewage have had for their object the manufacture of a solid manure, rather than the purification of the effluent water. Nor is it to be wondered at that constantly renewed attempts should be made to extract, from what is worse than refuse, some portion of the immense money value that sewage is so well known to contain.

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In reviewing the several processes that have been brought before the public, it is easily apparent that the greater number rely upon the precipitation and collection of the solid or suspended matter from the sewage; as though the impression existed, that this solid matter constituted the major part of the manurial value available. Some even of the most recent attempts appear to be based on this assumption; and although the desired result has been attained in collecting, into a portable form, the solid constituents of the sewage, and in rendering the effluent water transparent and inoffensive, it has been found that the nitrogen, the chief manurial component of the sewage, has, so far as the solution was concerned, not been in any degree appropriated in the process. In short, not more than $\frac{2}{15}$ths of the total money value have been secured, and it does not appear that in any case this amount has been found to produce a commercial return sufficient to defray the cost of manufacture, and to pay a fair interest on the capital outlay for the construction of works.

It is not proposed to consider the details of the several modes of dealing with sewage in order to extract its manurial constituents; a few of the principal inventions that have been tried will be previously reviewed, so as to ascertain what measure of success each has gained.

The Lime Process was tried many years ago. It has been adopted at Blackburn, Leicester, and Tottenham, and is reported on by the Rivers Pollution Commissioners[1] as being in all those places a failure, whether as regards the manufacture of a valuable manure, or the purification of the offensive liquid. The process consists in mixing a certain quantity of milk of lime with sewage as it arrives at the works; then by subjecting the mixture to violent agitation by means of machinery, and allowing it afterwards to pass into large subsidence tanks. In these reservoirs, the mud, composed of the suspended matter and lime, is deposited, and the supernatant water flows away into the nearest watercourse. The mud is afterwards raised from the reservoirs and deposited in pits, where it dries and is afterwards sold for what it will produce. The effect of the process, when properly conducted, is to clarify the sewage to a considerable extent. About 90 per cent. of the organic pollution is removed by the operation, but as regards the soluble impurities the effect is not so satisfactory; the amount of solid material in solution not being

[1] *Vide* "Report of the Commissioners on the Mersey and Ribble Basins," p. 52. Folio. London, 1870.

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reduced in amount, and at times it is even increased by the lime dissolving some of the suspended matter.

The analyses made by order of the Rivers Pollution Commissioners demonstrate that "the material which is of the greatest importance to remove from the dissolved constituents of the sewage, that is the nitrogenous organic matter, which enters rapidly into putrefaction,"[1] remains to a great extent in the sewage after treatment. Table 1 illustrates the average extent to which the lime and other processes have been found to succeed from a number of experiments.

What is known as the A B C process has lately been introduced; and professes to free sewage of its "organic and other impurities." This mode of treatment has been tried at Leamington under somewhat unfavourable circumstances; but at Hastings every facility has been afforded for making a fair trial, which, unfortunately, appears in this instance to have failed. Within the last few weeks the Metropolitan Board of Works have decided to permit the company to erect works at the Southern Outfall, where 500,000 gallons will be acted on daily. The materials used in the operation are alum, blood, clay, magnesia, as carbonate or sulphate, some compound of manganese, burnt clay, chloride of sodium, animal and vegetable charcoal, and magnesian limestone. But of these ten substances, it is stated that as many as six may be omitted.[2] Without any desire to criticise the composition of the mixture employed, or the mode of using it, it must be confessed that the nature of the combination of these ingredients with the soluble constituents of sewage does not at first sight appear evident. These substances, as described by the patentees, are mixed together, and added to the sewage to be purified until a further addition produces no further precipitate. The sewage is then thoroughly mixed with the compound and allowed to flow into the settling tanks, where the sediment is left to subside, and the water to flow away into the sea or river. It is supposed that the sediment when pumped up and mixed with a fresh supply of sewage exercises a further purifying effect. The solid material, which is dried by rotation in wire gauze drums, is then ready for the market. The analysis of the sewage before and after treatment shows that about 92 per cent. of the suspended impurity is removed by the process. Of the matters in solution no change of any value is found to take place; the amount of material is actually increased in proportion to the quantity of soluble mate-

[1] *Vide* "Report of the Commissioners on the Mersey and Ribble Basins," p. 55.
[2] Specification of Messrs. Sillar and Wigner.

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rial added, and as for the putrescible matter, the amelioration effected amounts to a reduction of only 55 per cent.

A modification of the lime process has been adopted at Northampton. In addition to the lime, chloride of iron is employed, in the proportion of 6 gallons to every million gallons of sewage. The lime is first added, and then the chloride of iron, and the mixture is subsequently filtered by ascension through a layer of calcined iron ore 8 inches thick, which separates the flocculent matter from the liquid. The deodorization resulting from this process is for the time being complete; but it is found that, although the effluent water enters the river in a clear and apparently inoffensive condition, the process of putrefaction is not permanently arrested. For some distance down the river the water appears to be unpolluted by sewage, but further on the condition is such as to afford substantial ground for complaint; and an injunction has been granted by the Court of Chancery restraining the Improvement Commissioners from discharging the sewage of the town into the river Nen. As is the case with the carbolates of lime and magnesia so it is with the chlorides of iron: the process of decomposition appears to be arrested for a certain time only, and after the use of such agents sewage becomes almost as offensive and prejudicial to health as without their use.

A phosphate process has been lately introduced by Drs. David Forbes and Astley P. Price. "This process is founded on the fact that certain mineral phosphates easily obtainable, especially those containing alumina, when in a hydrated or freshly-precipitated state, eagerly combine with the organic matter contained in the sewage: it being sufficient merely to agitate them in the most fœtid sewage to deprive it of all its odour and colour, even if tinctorial substances of great intensity be present in the solution at the same time; whilst the phosphate of magnesia combines with the ammonia contained in the sewage and precipitates it also in the state of double phosphate of ammonia and magnesia."[1] The mineral phosphate of alumina proposed to be employed exists in large quantity at the island of Alta Vela and in numerous other small islands lying to the south of St. Domingo. The deposit underlies a layer of guano, and is evidently of organic origin, with the remarkable peculiarity of containing little or no lime. The material is calcined, then treated with crude sulphuric or hydrochloric acid, which reduces it to a solution, and

[1] Paper read before the British Association for the Advancement of Science.

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in this condition it is added to the sewage. After the admixture is complete the phosphate is precipitated in the hydrated form, along with the organic matter in the sewage, by the addition of such a proportion of milk of lime as will neutralize the acid that holds the phosphates in solution. The deposit rapidly subsides and the supernatant water is then run off.

Experiments recently tried at Tottenham and at Bromley prove the efficiency of this method so far, but it is not claimed for this process that the effluent water is rendered very pure. The greater portion of the ammonia remains in solution, but much of the smell is removed; and samples that have been kept for months have not shown any tendency to fermentation or putrefaction.

It would be premature to express an opinion as to how far this process is likely to prove useful in practice; but it appears to possess certain advantages:—

1st. That the resulting manure, unlike that from the other artificial processes, is really of value. The analyses of Dr. Voelcker show that the deposit is worth about £7 7*s*. per ton,[1] whilst he estimates the manure produced by the A B C process at 19*s*.[2] per ton; and

2ndly. It is considered, with some justice, that the addition of an excess of the phosphate solution would supply to the sewage that phosphoric acid in which it is deficient, and so increase its manurial value.

In estimating the value of this, or any other, process as a means of purifying sewage, the point to which attention must first be directed is the degree of purification effected in the effluent water. As a commercial speculation the manufacture of solid manure from sewage is an important step gained; but for corporations whose duty it is to see that the sewage of their district is discharged into the rivers in an innocuous condition, the purity of the effluent water will ever be the first condition to be fulfilled. Possibly the process here described may, as an accessory to irrigation, prove valuable; but this experience can alone demonstrate.

The foregoing methods of treating sewage, although classed under the general head of 'Chemical' for the sake of convenience, are really to a great extent mechanical in their action. The mechanical process of clearing beer, wine, and other liquids, by means

[1] Analysis by Dr. Augustus Voelcker.

[2] Journal of the Royal Agricultural Society. Second Series, vol. vi., p. 415. 8vo. London, 1870.

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of a coagulum, is very similar to that of precipitating the solid matter from sewage. Without treatment sewage will continue in a turbid and gelatinous condition, but the addition of certain substances, especially those of an astringent kind, produce a coagulum, which lays hold of the suspended matter in the sewage, and carries it to the bottom of the reservoir by its weight.

When all the more effectual modes of chemical treatment are examined, they are found to fall short in their action of the desired result. The largest amount of organic nitrogen extracted in any case from the soluble constituents of sewage by chemical treatment has been about 66 per cent.,[1] which was accomplished by the lime process. Whilst in the use of Holden's process—a mode of treatment by sulphate of iron, lime and coal-dust, which need not be described in detail—the organic nitrogen in solution was increased by the materials used in the process dissolving some of the suspended matter. The chemical modes of treatment are not found to be perfect in their action, even so far as the removal of the suspended matters. Holden's process alone removes the whole of the insoluble materials, and the four other methods extract an average amount of 86 per cent. only.

Mechanical.

The only purely mechanical mode of treating sewage is that of filtration, either downwards or by ascension. This process has not come into actual use except at Ealing, where neither are the conditions favourable nor the results satisfactory: a certain degree of clarification has been accomplished, but, in effect, the partially purified sewage is productive of nuisance, and its impurity is manifested by the presence of the sewage weed in the stream below the outfall.

The experiments on filtration made in the laboratory of the Rivers Pollution Commissioners are calculated to engage the serious attention of engineers, as being an approximation to the much desired result. It is not a necessary inference that a laboratory experiment, or a series of such experiments, is to be taken as conclusive evidence of the success of any process in actual practice: so much indeed is admitted by the Commissioners in their Report, and the possible causes of failure are anticipated by them. The filtration of water for the supply of towns dates

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[1] *Vide* "Report of the Commissioners on the Mersey and Ribble Basins," p. 94.

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from great antiquity, but until lately the whole of the purification hoped for in the process was the removal of matters held in suspension. It appears to be generally admitted by waterworks engineers that the process of filtration is partly chemical, partly mechanical; and that the soluble impurities may be for the most part removed or altered by the process of filtration through sand. Professor Wanklyn states that an examination made by him of the waters of the Thames collected at Hampton, whence it is taken for the supply of the metropolis, gave 15 parts of albumenoid ammonia in 100,000,000 parts of water. He examined the same water after its passage through the filtering beds of one of the water companies and found the impurity was reduced by the process to one-third. The experiments made on the purification of sewage show, in a still more remarkable degree, that the operation of filtering is almost as much a chemical process as it is a mechanical one; the different soils used as filtering media producing great disparity of result. The first set of experiments, to quote the words of the Report, "was made with two long glass tubes charged, the one with sharp silicious sand, the other with a mixture of equal parts of sand and coarsely-powdered chalk. The London sewage, from the outfall of the Regent Street and Victoria Street sewers in Scotland Yard, was passed continuously upwards through one of these tubes so as to exclude aeration, and subsequently downwards in successive doses through both, so as to cause the air to follow the sewage down the tube between each two consecutive doses. The effluent water was submitted to analysis once a week."[1] The results of these experiments on upward filtration are by no means satisfactory. Of the impurities held in solution but a small percentage was removed, and their substitution by nitrates and nitrites—which represent the putrescible impurity in a converted form—was inconsiderable. The first essential condition for the filtration of sewage, and probably also for the filtration of drinking water, is that it shall be intermittent, so that the interstices of the filtering material shall have time to become aerated. This is not easily practicable with upward filtration, and clearly accounts for the imperfect results attained by the process. The operation of downward filtration, when carried on through different kinds of soil, produced results that may be regarded as very remarkable. The apparatus used consisted of glass tubes 6 feet long and 10¼ inches in diameter: these were placed upright in earthenware pans and were filled

[1] *Vide* "Report of the Commissioners on the Mersey and Ribble Basins," p. 62.

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nearly to the top with the soils that were used in the experiments. A sufficient space was left at the top of each cylinder for the reception of the sewage, of which equal quantities were added night and morning. The average composition of the sewage was ascertained at the commencement of the process, for comparison with the effluent water. The degree of purification appeared to depend to a great extent on the rate at which the sewage was passed through the filtering material. Without going into particulars as to the purification effected in each case, it will be sufficient to state, that in every instance the whole of the suspended matter was intercepted; and of the soluble organic pollution three of the soils examined removed an average amount of 96·5 per cent. of organic nitrogen, and three other soils gave an average of 88·1 per cent. of the same material removed.

Before considering the applicability of this process to practice, it will be important to anticipate what would naturally be offered as an objection to its adoption. It would be reasonable to suppose that, sooner or later, filtration through sand or other material would be interrupted by the pores becoming clogged; but such a conclusion does not appear to be justified by the results, for after passing samples of sewage for three months through one of the filters described, and after "a long series of experiments, there were no symptoms of clogging or diminution of activity, and the effluent water was always bright, inodorous, and nearly colourless."

As a practical conclusion from these experiments the Commissioners give it as their opinion, that "with properly constituted soil, well and deeply drained, nothing more would be necessary than to level the surface and divide it into four equal plots, each of which in succession would then receive six hours' sewage. In this way the sewage of a town of 10,000 inhabitants could at a very moderate estimate be cleansed upon five acres of land, if the latter were drained to a depth of 6 feet."

There is one objection to this process, viz., that the whole of the valuable portion of the sewage is wasted, and such being the case, the process only accomplishes in part the desired object. Notwithstanding this there are situations and conditions that appear almost to justify waste even on as gigantic a scale as this process implies. In localities where land cannot be procured of sufficient extent to employ irrigation, it is possible that downward filtration may be made available and worked successfully. It has been seen that as a rule deodorizing systems are worse than wasteful, for besides giving little or no return, they entail heavy working

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expenses, and are often very offensive in their operation. When regard is had to the undoubted fact that they do not effectually purify sewage it may be hoped that a substitute will be found in filtration.

Irrigation.

The great preponderance of scientific evidence goes to prove the success of irrigation with sewage, whether it be regarded merely as a mode of abating nuisance on a great scale, or as a commercial speculation. There are, as will be seen, certain authorities, whose opinions and learning are entitled to respect, who take exception to this system; but as against these there is almost the whole weight of experience to decide the matter in favour of irrigation, this process will probably hold its place, until what appear now to be little more than anticipations are to some extent supported by tangible proof.

Before entering on the details of the arrangement of sewage farms, it will be well to dispose of three questions that are suggested at the outset:

1st. Is irrigation an effectual mode of disposing of sewage?
2ndly. Is it prejudicial to health?
3rdly. Is it economical and profitable?

A process to be effectual must provide for the removal of the noxious or putrescible constituents of sewage without creating a nuisance, and must render the effluent water of sufficient purity to admit of its being discharged into the watercourses of the country.

Much difference of opinion exists with regard to this point, some authorities maintaining that absolute purity from organic matter is essential, whilst others contend that, so long as the senses do not detect the impurity, its presence is of little importance; in other words, that water devoid of offensive smell, and that is clear and colourless, is sufficiently pure to be admitted into running water. The Royal Commission, in their recommendations relative to the Mersey and Ribble basins, suggest four standards for determining the purity of effluent water resulting from ordinary sewage, as distinct from that resulting from special manufactures.

With ordinary precautions, irrigation is capable of purifying sewage sufficiently to meet each one of these standard degrees. It is necessary, however, to premise, that adequate care should be taken in the laying out of the surface, of the drainage, and of

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other matters necessary for successful sewage farming; otherwise, as is the case in some few instances, the purification may be accomplished in part only, and the system thus be brought into discredit.

Taking the instances of Banbury, Warwick, and Norwood, as fair examples of the application of sewage to clay soil, the analyses of the effluent water give for the three towns an average result of organic carbon equal to 1·32 part in 100,000, the quantity of organic nitrogen in the three examples being 0·221 part in 100,000. Taking three favourable examples of irrigation on a porous soil, as Croydon, Penrith, and Carlisle, it is found that the effluent water contains of organic carbon only 0·51 part in 100,000, whilst the average quantity of organic nitrogen in the three instances is 0·146 part in 100,000. Without multiplying examples, these six cases show that irrigation, when managed with reasonable care, removes effectually a sufficient quantity of the putrescible matter to render the discharge of the effluent water into rivers devoid of objection.

In each of these examples, with the exception of Banbury, the whole of the suspended matter is removed. At Banbury, though the management is unfavourable for a high degree of purification, 93 per cent. of the suspended matter is removed, the remaining 7 per cent. probably escaping through fissures in the surface.

It has been said that the purification of sewage is arrested in wet weather, and during prolonged frost. During heavy or continued rain the process of absorption may perhaps be to some extent suspended, but it rarely occurs that rain is so heavy as to flow off the surface of cultivated land; and as this usually takes place when the watercourses of the country are swollen, it follows that any sewage that may be carried away unpurified is so largely diluted as to be innocuous. It will be well for those who regard this as an objection to sewage irrigation to recollect that precisely the same thing may, and frequently does, take place in ordinary farming; manure, it may be, consisting of town refuse, and containing a large quantity of excreted matter, is laid down, and before there is time to plough it into the ground a shower may descend and carry away a great deal of what is very impure into the nearest watercourse. Being, however, largely diluted, this matter passes away without producing injury of any kind. To enter upon the general question here, whether the streams that traverse cultivated and populous districts are to be regarded as fit sources of supply for drinking water, would entail too wide

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a digression. One fact remains perfectly clear, that if the effluent water from a sewage farm introduces animal organisms or ova calculated to produce specific diseases, the same result may occur from the use of ordinary refuse as manure.

Experiments made to ascertain the degree of purification effected during frost show, that the detergent functions of vegetation are to some extent suspended during a prolonged period of cold; but although the extent of the purification is less than in temperate weather, it is still considerable; and, so far as the investigation has gone, the organic nitrogen in the sewage has not been more than about one-fourth of the amount originally in solution.

In estimating the amount of impurity in the effluent water from a sewage farm, it is necessary to bear in mind that the quantity of the effluent water is much less than that of the sewage that reaches the farm; a large amount of the water is generally absorbed and evaporated in its passage over and through the soil; and, to arrive at a just estimate of the degree of purification actually effected, it is necessary to compare the amount of organic impurity that remains in the effluent water with the quantity of sewage actually arriving on the ground, and not with the amount of effluent water.

The impression that sewage farms are prejudicial to the health of the neighbourhood is fast dying out. In the absence of experience, it would be reasonable to infer that the discharge of a vast quantity of excreted matter over the surface of the ground would produce formidable disease, but that such is not a sound conclusion there is abundant evidence to prove. Even at Edinburgh, where the quantity of land employed for irrigation is disproportionately small, and where it must be admitted the purification of the sewage is in consequence by no means complete, it is shown, so far as a negative proposition admits of proof, that the public health has not suffered in any material respect.

The Author's experience in India, were it not supported by other testimony, shows that irrigation, as practised in that country, affects health to an appreciable extent. The mortality returns of certain districts have furnished proof that the establishment of irrigation works has somewhat increased the death rate, and the same influence is felt in Northern Italy and elsewhere. It must be borne in mind, however, that the systems that produce these results are not similar to the ordinary mode of applying sewage to land. In one case the ground is warped, or completely submerged, for days together, whilst sewage should be suffered

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only to trickle over the surface for a few hours at a time. A medical authority of eminence[1] holds, that irrigation fields must prove the medium of propagating entozoa, or parasites, that in certain stages of development produce formidable disease. This theory, if substantiated by experience, would form a reasonable objection to sewage irrigation, but, fortunately, there is not a particle of evidence to show that cattle fed on sewage farm produce are subject to any special forms of disease. It has for ages, and in all countries, been the practice to use the excretal matter of man and other animals as manure; and surely, if such a custom has not been found to favour the development of parasitic life, no anxiety need be felt that the application of sewage will do so. Let it be admitted, however, for the sake of argument, that these living germs are a source of danger, and there is in carbolic acid a remedy that, used in minute quantity with the sewage, will insure their destruction without in any way impairing the efficacy of the sewage as a manure. After a series of most instructive experiments, the Cattle Plague Commissioners have reported that "the tar acids act most powerfully in arresting all kinds of putrefactive changes, and annihilate with the greatest certainty all the lower forms of life."

In entering upon the third inquiry, as to whether irrigation is an economical mode of dealing with sewage, a variety of circumstances will demand consideration: the cost and the situation of the land, both as regards distance from the town and as to physical position, will first attract attention; it being evident that, other things being equal, gravitation works will have a great advantage, as compared with works in which it is necessary to employ steam power to raise the sewage. The fact of the farm being situated near to, or remote from, a suitable market for the produce to be grown, will also affect the question of economy in an important degree; and it is only by taking all these circumstances into account that the best result can be arrived at. It is not always possible to select land solely with regard to its physical position, for a variety of considerations may present themselves to induce the engineer and his clients to sacrifice something to local circumstances.

It is not desirable to establish a sewage farm to the south-west of a considerable population, as it is from that point of the compass that the winds prevail in this country. Again, it is well to avoid the direction in which building operations are likely to develop; and further, experience teaches that, if corporations hope to succeed

[1] Dr. Spencer Cobbold, F.R.S.

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in acquiring land for their sewage, they will act wisely in avoiding collision with influential landed proprietors. The opposition likely to originate with the owners of property does not necessarily arise out of the apprehension that the health of the neighbourhood is likely to be seriously affected; they feel, not unnaturally, that, whether the evil is substantial or sentimental, the value of their property is likely to be prejudiced in the estimation of the public. It is thus evident that the circumstances are, indeed, rare under which an engineer can select the best possible position for his outfall works. It is for him to choose the position least open to objection, rather than that which the simple expediency of engineering practice would suggest.

It is worthy of remark, that in almost every case corporate bodies seek for land in close proximity to their town; and, although this may at first sight appear judicious, it is really in many instances the reverse. The main question that affects the economy of the process is the price paid for the land; and a little consideration will show, that the reduced value of land remote from towns will often more than countervail the extra cost of the outfall sewer; and it may be, and often is, more economical to select land at a considerable distance from, or at a higher level than the district drained, even though pumping should become a necessity. Taking, for example, London, or any large provincial town, it will be found that all the land in the immediate neighbourhood will command high prices for building purposes, whilst at a distance of a few miles an almost unlimited quantity of land may often be secured at little more than agricultural prices.

Reviewing all the circumstances, it is clear there must be a great variation in the results to be obtained in sewage farming as a means of income; and probably the only way to arrive at a sound estimate is to refer to what has already been done, and see what profits have been realized in instances where sound judgment has been exercised. Selecting, for example, Breton's farm near Romford, which receives the sewage of that town, it has been found worth while to pay the local authority the sum of £600 per annum for the sewage alone, or at the rate of about 2*s.* per head of the population that furnishes it. A further sum of £3 per acre is paid for the land, which is a poor gravelly soil, almost unproductive without the addition of large quantities of manure. On this farm the minimum crop is found to sell for £20 per acre, and three or four such crops can generally be secured in the course of a year. All kinds of vegetables have been grown on this farm, realizing in almost every case considerable returns; lettuce, cabbage, and onions, selling respectively for £30, £32, and £46 per

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acre, and this when preceded or followed by other crops in the same year.

At Lodge Farm, near Barking, the cultivation of ordinary vegetables and cereals has proved equally successful and remunerative; and Italian rye-grass has been produced at the rate of from 60 tons to 70 tons per acre in the year, and sold on the ground uncut for £1 per ton.

A few years ago, it was thought that Italian rye-grass was the only crop that could be raised under the application of sewage; and its wonderful capacity for liquid manure makes it one of the most valuable accessories in the management of a sewage farm, where the purification of the liquid is of the first importance. It is also a most valuable crop, not only as regards the amount of nutriment it contains, but from its coming in earlier and continuing later than almost any other green crop. The use of rye-grass must, however, be limited by the demand for it, and it has been found, in some situations, that there was not a sufficient market to make it as profitable as was hoped. This has been urged as an argument against the adoption of sewage farms with a view to profit; but since it has been discovered that the application of sewage to nearly all vegetables, as well as to cereal crops, is attended with success, the objection is at once removed. It is hardly too much to say that sewage, if it does not accomplish all that farmyard manure will, can be made in its application quite as general. It is not necessary that the land should always be cropped in order that the liquid shall be purified; for whatever the theory of the process may be, it is well known that the soil will of itself take up and store the manurial constituents of sewage to a great extent, and impart them again to the first crop that is grown.

Time will not admit of carrying the consideration of this subject into all the details of sewage farming. Such minutiæ are of the first importance if a high degree of success is to be achieved, but for the present purpose it will be sufficient to consider some of the general principles to be kept in view. Though the services of the engineer will be required for the arrangement of works, the determination of levels, drainage, capacity of conduits, and a variety of other matters, it is evident that his functions become intimately associated with those of the scientific agriculturist, whose co-operation he will, if judicious, avail himself of. Whatever land be selected for the disposal of sewage, it will have to be reformed; it will be necessary to lay out suitable approaches, and the whole farm will, as a rule, have to be under-drained.

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At the outset it may be premised that almost any kind of land, if properly treated, will be suitable for the reception and purification of sewage. Existing examples afford ample proof of this fact. At Aldershot, on the Bagshot sands, the soil is perhaps as unpromising as it is possible to conceive, consisting of almost pure silica, resting on a dense layer of peroxide of iron. Without the assistance of sewage this land is practically valueless, but under judicious management it has been let for £20 per acre. At Edinburgh, the soil to which the sewage is applied is simply sea sand that has been drifted above high-water mark, wholly unproductive of itself, but now realizing £25 per acre. Proceeding to the opposite end of the scale, it is found that the stiffest clay soils purify sewage completely. The chemical composition of such soils indeed adapts them for the purification of sewage; and it is remarkable that there is no more general fallacy, than that stiff soils are unsuited for sewage farming. Viewing the soil merely as a chemical purifier of sewage, clay seems to have the advantage over lighter soils; but in practice this is not apparently verified. Plate 19 shows unmistakably the superior purifying effect of the light soil of Beddington over the clay of Norwood.[1] Probably in some instances the comparative excess of organic nitrogen, found in the effluent water at Norwood, was due to some of the sewage escaping through land cracks; but the diagram indicates a perceptible uniformity in the amount of nitrogen at different dates on the two farms, and this can only be accounted for by atmospheric changes, which facilitated or retarded the purifying power of the soil. In stating that any soil may be made available for the reception of sewage, a reservation should be made in the case of low-lying and water-logged ground; such land nearly always produces a certain amount of miasmatic exhalation, and could only be rendered more unhealthy by the addition of sewage. But even this reservation is one that depends on expense, for marshy ground, when thoroughly drained, can be made as available for sewage farming as any other.

Sewage may be utilized with one of two objects in view; first, for the abatement of the nuisance that it would otherwise produce, and secondly, with the design of securing the largest amount of profit that it is capable of furnishing. Whichever of these views prevails will determine the quantity of land required for a given population; as a much less area of land will suffice merely to render sewage innocuous than would be required were it only desired to

[1] Analysis conducted at the College of Chemistry.

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turn the manure to the best possible account. Opinions as to the area of land requisite for the disposal of sewage vary within very wide limits, and appear as a general rule to be based upon some empirical mode of reasoning. The sewage of two hundred people has been effectually purified on 1 acre of land at Carlisle, whilst engineers of eminence have stated that the sewage of from five to seven people is sufficient for the manuring of a single acre.[1] The Author's experience is opposed to the application of sewage in small quantities, as the difficulty of distributing the liquid is very great; but much will of course depend on the dilution of the sewage, by wasteful consumption of water, or by the addition of subsoil water.

The experiments on downward filtration, already alluded to, demonstrate the fact that very small areas may, by proper disposal and judicious under-drainage, be made to serve for the purification of sewage, which is the primary object to be aimed at; but it is not therefore desirable to employ the minimum quantity of land that will serve the purpose. To apply sewage to land with economy and profit, it is necessary to secure a sufficient extent of ground. It is obviously impossible to establish an invariable rule for the due proportion of land to a given population; for, in the first place, the composition of sewage in different towns varies within very wide limits; and secondly, the purifying qualities of land will be found far from uniform. Plate 19 illustrates how great is the difference in the purifying powers of the stiff clay soil of Norwood and the free soil of Beddington. Fig. 2 illustrates the quantity of organic carbon remaining in the effluent water as it passes from the land. The dark zigzag bands are plotted from analyses made at intervals of about a fortnight throughout a whole year; and represent the total amount of organic carbon in the sewage on its way to the farm. The lower, or dark one, shows the amount of organic carbon remaining in the effluent water at Beddington, whilst the lighter band shows the proportion of the same impurity found in the effluent water from the Norwood farm. This diagram is only intended to illustrate the relative amount of the impurity removed in each locality. The light shading represents the organic carbon in the South Norwood sewage, which contains 50 per cent. more than the Beddington sewage. For the purpose of comparison, the amount of impurity

[1] Evidence given before Select Committee on the Sewage of the Metropolis, pp. 111 and 117.

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in the Beddington sewage is brought up in the same proportion as the organic carbon in the unpurified sewage of Norwood; or, in other words, it is assumed that the Beddington soil would effect an equally good comparative result if it had to purify sewage of the same strength as that of Norwood.

Fig. 1 shows the amount of organic nitrogen in the average sewage of both Norwood and Beddington, and it is remarkable that the impurity should be almost exactly the same in amount in both examples, though there is a wide difference in the respective circumstances of the two places. The upper band shows the amount of organic nitrogen in the effluent water at Norwood, whilst the lower band shows the relative quantity of the same material in the Beddington effluent water. It will thus appear at a glance what a diversity of effect may be produced by different soils. The balance in these two cases is greatly in favour of the free open soil, though chemical theory would seem to indicate a clay soil as relatively the best purifier of sewage.

If it is intended to utilize sewage with a view to profit, there is no guide but experience to indicate how many people of all ages will, under ordinary circumstances, furnish sewage enough for an acre of land. The balance of opinion indicates a proportion of one hundred people, under ordinary circumstances, to 1 acre of average land. It has been seen that the sewage of double this number of persons has been applied to land, and been perfectly purified on 1 acre. No doubt the value of the water alone applied to land is considerable, and a much larger experience, and more careful experiments, alone can determine the area necessary to produce the best result. Regarded as to its manurial qualities alone, however, it appears that when the proportion of as many as two hundred people to the acre has been adopted, the whole of the most valuable part of the sewage was either taken up by the plant, or retained in the soil, the effluent water showing but a trace of nitrogen in any form.

One of the first considerations in laying out a sewage farm will be the mode of distribution to be adopted; and this is a matter upon which there is considerable difference of opinion. The simplest method of distribution is that called catchwork. It consists in cutting on the surface of the land, which must have a considerable slope, a series of distributing channels following the contour of the ground. In this system the water from one channel flows over the surface until it reaches the channel next below it, which acts as a catchwater drain. This channel again

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allows the sewage to overflow and pass over the surface to the next lower channel, and so on to the bottom of the land. In the simplest kind of catchwork, the lower portions of the farm receive sewage of reduced strength from that discharged on the upper ground; and though it is doubtless desirable, that the quality of the sewage should be as uniform as possible all over the farm, still, if purification be the great desideratum, it is evident that the liquid must be passed over as much of the land, and as frequently, as may be necessary to complete the process. It is not, however, essential in this system that the sewage, when reduced in strength, should be again used; for by cutting a series of carriers at right angles to the contour lines, the sewage can at once be delivered at any level, and be discharged over any part of the surface at pleasure. It is evident that the catchwork system will commend itself on the ground of economy, but it is only applicable in situations where the land is of an undulating character, or presents such an inclination as will admit of a regular, but not too rapid, discharge. If the inclination be too great, the sewage will pass away so rapidly as to leave an undue amount of the fertilizing matter unappropriated by the soil.

The system of irrigation known as pane and gutter is applicable to land having but a slight surface inclination, and is both cheap to carry out and easy to manage. The main carrier is so situated that it will command the greatest possible area, consistent with an inclination that will deliver the sewage to the distributing channels. From this main channel the distributing carriers start, following generally the direction of the greatest fall: from these, again, smaller cuts may be made at right angles, or nearly so, so that the whole has on plan a herring-bone appearance. The smaller distributing channels are laid out nearly level, in order that the sewage when allowed to enter them may be dammed back by boards or stops, until it flows in an uniform stream over the surface.

A third system of surface irrigation is that known as the ridge-and-furrow, or bed system, which is the only one applicable to very level lands. It is more purely artificial than either of the other two, and is the most refined system of surface irrigation practised. The ground is laid out in long rectangular beds; and these are shaped either by the plough or by manual labour, so that the centre line, traversing the crest of the bed, is raised to form a ridge. Along the top of this ridge there is cut a channel having a very gentle slope, and on each side, or in the hollow between two of these beds,

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there is a channel for the removal of the water after it has passed over the surface. The flow of the sewage in the upper channel is controlled, as in the other systems, by stops of wood or iron, and the sewage flows with perfect evenness over the edge of the channel.

It is hardly necessary to describe here the actual mode of forming the surface in this or either of the other systems. It may suffice to say, that much may be done with the common plough and the subsoiler, though a certain amount of levelling and dressing of the surface must necessarily be performed with the spade. It is not always possible to lay out the beds so that the ridge shall have a perfectly uniform inclination, but this can be met by disposing the carrier in steps, at which points the stops may be placed. The length of the beds in this system is not a matter of great importance, so long as they are approached by a road at either end; though it will be more convenient, and generally cheaper, to restrict the size than to aim at making the beds very large. As regards width and inclination, practice will form the best guide. At Barking farm the largest are beds 150 feet wide, the elevation of the ridge being about 1 foot, which is found to be quite sufficient. At Breton's farm, which receives the sewage of Romford, the owner—after a number of careful experiments, to ascertain what was the best and most convenient width of bed—has determined on adopting 30 feet. "This breadth combines other great advantages not connected with irrigation; for the area of a bed is at once obtained in square yards by multiplying the length by 10. This for selling market garden crops is of very great convenience."[1] Besides the convenience of narrow beds, they possess the advantage of being more cheaply formed, and effect a marked economy in the disposal of the sewage.

The rules laid down by some of the best French authorities on irrigation as practised in France and Belgium are, that the minimum slope of the surface shall not be less than 1 in 125, that the greatest inclination *may* be 1 in 3½, and that slopes varying from 1 in 10 to 1 in 33 will be found the most convenient in practice.[2] It is not possible to establish any general rule for the laying out of irrigation ground, so long as engineers have to deal with land

[1] "Lecture on Sewage Irrigation, delivered by W. Hope, Esq., to the Ratepayers of West Derby," p. 14.

[2] "Irrigations, Engrais Liquides et Améliorations Foncières permanentes." Par M. Barral, p. 402.

Vide "Manuel d'Irrigation." Par M. Jules Deby, p. 81.

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varying in quality and inclination; each case must be treated according to circumstances by one of the foregoing methods, and more in accordance with the existing features of the surface than by any general rules.

Until recently the usage has been to distribute sewage in open carriers or cuts on the surface of the ground; but from their irregularity, and from the constant soakage into the soil, these channels were always more or less offensive. Now, however, when any degree of refinement is aimed at, carriers of iron, earthenware, or concrete are used, which necessarily discharge the sewage without leaving any considerable portion to adhere to the sides, and cause foul smell. Plate 20 shows a variety of carriers adopted in different localities, including the wrought-iron carrier supported on wooden trestles, as used at Breton's farm.

It is not proposed to discuss here either the method of subsoil irrigation or that known as the hose-and-jet system. The first of these is beset with disadvantages, and the second is inapplicable on a large scale. In many instances the hose-and-jet plan has been practised with great success and profit, but it would be found unmanageable and expensive on such a scale as the application of the sewage of towns and cities necessarily involves. The ruling maxim in applying sewage to land is, that the works of distribution shall be simple and easily controlled—a principle that hardly applies to either the hose-and-jet, or the underground system of irrigation.

A point worthy of consideration in applying sewage to land, is the degree of filtration that it is desirable to submit the liquid to before allowing it to flow into the channels. A variety of insoluble matters are to be found in all sewage, and a large proportion of the whole will consist of solid excreta. This portion, which is really the most offensive to the senses, contains only about $\frac{1}{7}$th part of the fertilizing material. It is usual, in ordinary practice, to allow almost the whole of this to leave the outfall, and distribute itself over the surface of the ground, or, rather, adhere to the sides of the carriers, but the expediency of such a course is questionable. Where the farm is remote from dwellings, no great inconvenience will arise from exposing a large amount of offensive matter on the surface of land, where before long it becomes deodorized by exposure to the air and soil; but in situations contiguous to dwellings it will certainly be desirable to arrest a considerable part of the solid material, and afterwards apply it as ordinary manure to the ground, or to dispose of it for what it will return, which as a general rule is not a large sum. At Norwood, where most of the ordure is removed by upward filtration, the material is mixed

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with burnt clay and used for filling up hollows in the surface of the ground; and at Ealing the entire of the solid matter resulting from a population of nine thousand five hundred is mixed with ashes and disposed of for about £75 per annum.

If it be desired to employ sewage without creating prejudice and distrust, it is most desirable that it should be clarified to a considerable extent; the loss is but trifling, and avoidance of all nuisances is certain.

The communication is accompanied by a series of diagrams, from which Plates 19 and 20 have been compiled, and by several Tables, containing further detailed information, which are appended.

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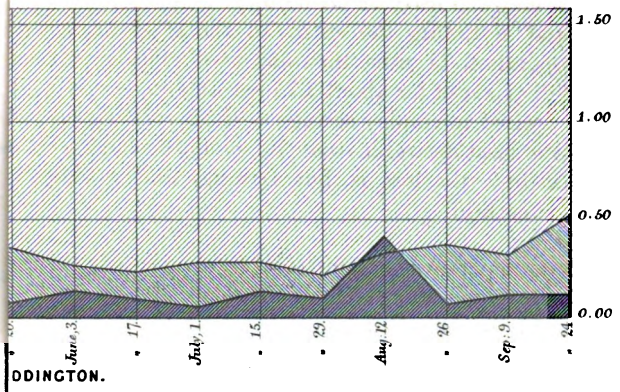

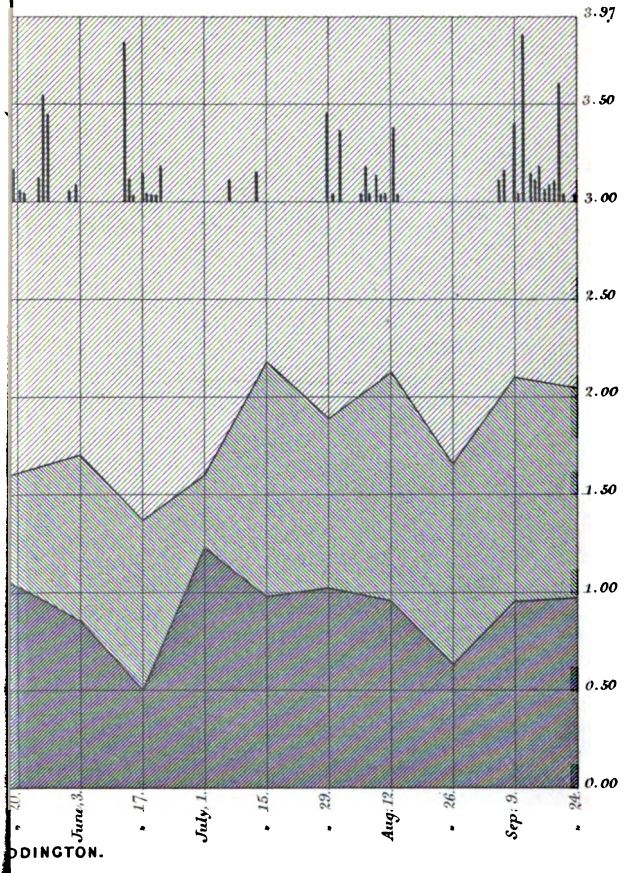

NOTE
The vertical measurements on the Scale of rainfall are equal to the depths that fell on each day.

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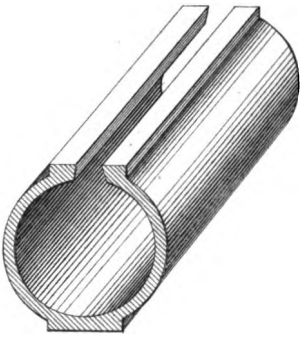
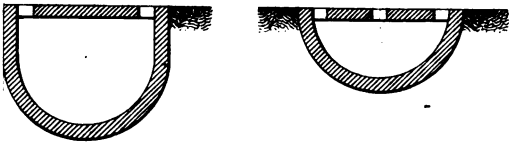
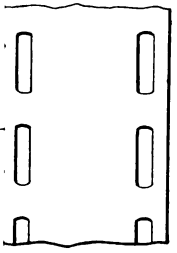
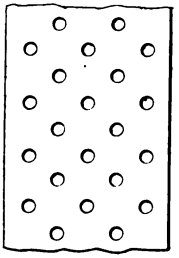


REDHILL.







NORWOOD.

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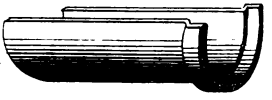

OPEN CARRIER.

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APPENDIX.

TABLE 1.

	Organic Carbon.	Organic Nitrogen.	Insoluble Matter.
CHEMICAL.			
The Lime Process.			
Best	36·1	65·8	96·8
Average	27 7	43·7	80·6
Worst	23·4	10·4	59·6
The A B C Process.			
Best	34·8	58·9	96·0
Average	32·1	54·3	92·0
Worst	25·8	50·1	87·4
The Northampton Process	50·1	37·1	99·8
MECHANICAL.			
Downward Filtration.			
Through sand, 5·6 gallons per cube yard in 24 hours.			
Best	84·9	96·9	100·0
Average	83·3	95·6	100·0
Worst	81·1	94·5	100·0
Upward Filtration.			
Through sand, 3·6 gallons per cube yard in 24 hours.			
Best	50·7	65·5	100·0
Average	26·3	43·7	100·0
Worst	00·6	12·4	100·0
IRRIGATION.			
Bedford light gravelly soil.			
Best	78·9	97·4	100·0
Average	71·6	81·3	100·0
Worst	60·5	70·8	100·0
Norwood clay.			
Best	76·1	92·0	100·0
Average	65·0	75·1	100·0
Worst	51·3	44·1	100·0

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TABLE 2.—Localities where Chemical and Mechanical Processes are employed.

LOCALITY.	Number of Inhabitants Contributing Sewage.	Cost of Purifying Works.	Annual Outlay for Working.	Mode of Purification Employed.	Annual Return from the Sale of Manure.	Remarks.
Alton	3,000	£800	£55	Lime and perchloride of lime.	Nil.	
Canterbury . . .	11,000	£2,000	£125	Charcoal	£15	A larger profit anticipated.
Ealing	9,500	£3,000	£300	Filtration through sand and charcoal with deodorization by tar and lime.	£75	It is estimated that the cost of these works is 50 per cent. higher than if they had been constructed in excavation.
Hastings . . .	25,000 to 30,000	£5,000 to £6,000	£3,000 to £3,500	A B C process, Sillar's patent.	28 tons of manure produced per week, and said to be sold at £3 10s. per ton.	Particulars furnished by the Secretary to the Company.
Luton	8,000	£3,000	£470	Higgs's patent (milk of lime).	£65	A much greater return expected.
Northampton . .	30,000	£6,000	About £500	Sulphate of iron, alumina and lime, perchloride of iron, and alumina.	£490	This process is about to be abandoned for the ordinary lime treatment.
Uxbridge . . .	6,000	£2,000	£180	Filtration through charcoal.	£25	Charcoal changed every 6 weeks in summer.
Worksop . . .	8,650	£5,800	£37	Lime process	£38	

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Surface Wat... with the... or...	...m the Application ...l Expenses ... for.	...ned or not.
Yes .	. . .	13
Yes . .	The ...down... to	145 a
Yes . .	...acre, or £ ...fit.	260 acres.
Yes . .	...4 15s. per ac...	...s.
Yes . .	...or 15 years at a nor... £5 per annum.	
Yes . .	...annum	
Yes . .	calculated at 5s. per ... for each person con... ...ng the sewage.	
No . .	...s nil; profits from £100 ...0 per acre on vegetables, ...20 to £29 on grass.	32 a
¼ of the rai... on roofs a... passed int...	...s nil; cabbages weigh ...56lbs. to 70lbs., mangold ...to 23lbs.	18 acre
No . .	...s ab... ...er annum.	19 acre
Partl...		
No .	£20 ... per ... Estimated ... per head of population.	

* Part of the ... of the two.

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Worksop . . .	8,650	£5,800	£37	Lime process	£38	

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Surface Wat with the or r	rising from the Application he Sewage, all Expenses being allowed for.	Area under Cultivation.	Land Underdrained or not.
Yes . .	present.	130 acres.	Underdrained.
Yes . .	The sewage is given to ndowners.	145 acres.	Not underdrained.
Yes . .	cre, or £260 per annum fit.	260 acres.	Partly underdrained.
Yes . .	4 15s. per acre net re-	30 acres.	Formerly underdrained; the pipes had to be removed.
Yes . .	r 15 years at a nominal £5 per annum.	100 acres. .	Not underdrained.
Yes . .	nnum	65 acres.	Underdrained.
Yes . .	calculated at 5s. per for each person con- ng the sewage.	.. ..	Underdrained.
No . .	s nil; profits from £100 0 per acre on vegetables, 0 to £29 on grass.	32 acres.	Partly underdrained.*
of the rai on roofs a passed int	s nil; cabbages weigh 56lbs. to 70lbs., mangold to 23lbs.	18 acres.	Partly underdrained.
No . .	s about £25 per annum.	19 acres.	Underdrained.
Partly comb	not known; crops excel-	121 acres.	Underdrained.
No . .	cellent; some of the land £20 per acre to cow- s. Estimated return per head of population.	113 acres.	Partly underdrained.

* Part of the of the two.

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Dr. H. LETHEBY expressed his disappointment at the manner in which the subject of the Paper had been treated. If he had been told that this was to be an abstract of the Report of the Royal Commissioners on the Pollution of Rivers he should perhaps have been prepared to have considered it in detail. With regard to the chemistry which the Author had brought to bear upon the subject he would say at once that it was not merely novel in style and nomenclature, but it was also very inexact in its conclusions; for it was derived from processes of analysis which were not generally accepted by chemists. In point of fact, in estimating the quality of sewage, whether it was to be distributed upon the land, or precipitated by chemical processes, practical men were accustomed to judge of it, not by the amount of organic carbon or organic nitrogen derived from processes that were altogether untrustworthy, but by the quantity of ammonia and other manurial elements contained in it; and the way of expressing its value was at so much per gallon instead of per 100,000 parts.

With respect to the other portion of the Paper, he was disappointed, because in the discussion of such an important subject by a member of a practical body of men he did expect to hear the most practical treatment of the question. Instead of this, however, he did not hesitate to say, from what he had heard of the Paper, that the Author of it had really had no experience whatever of the subject, and had not even visited the places which he ventured to speak of. To take one instance only: the Author referred to a process of precipitation of sewage at Northampton by means of chloride of iron, and he said that after filtration such and such results were obtained. Now it was more than twelve months ago that the process referred to had been abandoned and another adopted, namely, precipitation with crude sulphate of iron, without filtration at all. Many other remarks on the subject justified him, in like manner, in concluding that the Paper was not a practical Paper derived, as it ought to have been, from the actual experience of the Author; but was in reality a digest or abstract of the Report of the Royal Rivers Pollution Commission.

The question submitted to the meeting by the Author, namely, the respective values of the processes of irrigation and chemical precipitation, must be judged by practical results. Irrigation, if it were tested by its actual capabilities as regarded economy or sanitary results, would be found to be a great failure. In fact, he would ask the Author of the Paper—or rather he would put it to any one of experience—whether there was a single farm in the

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country where, after five or six years' working, the process had been found to be either economical or effectual in its results? He likewise appealed to anybody who had visited the irrigation farms of England whether the soil had not been overrun with sewage to such an extent as to render it a nuisance to every one in the neighbourhood of it; not merely to those living, as the Author expressed it, to the south-west of the place, but everywhere about it? And in the next place he asked whether the sewage distributed upon the soil was so effectually deodorized as to be admissible into the neighbouring watercourses without being a serious injury to them? On the contrary, it was the fact that in every one of such cases the neighbouring watercourses were found to be highly polluted by the water flowing from the land; and with regard to the question of economy, he would say that there was not a single farm that could be named except the filthy Craigentinny meadows, near Edinburgh, where any profit from sewage irrigation was derived. It might do for a year or two, especially in dry seasons; and during the last two years the weather had been so dry that probably there had been some success with sewage irrigation; but water alone without any manurial elements would have been under such circumstances equally successful; and he asked for large experience before a conclusion was formed. His own opinion was that not a single sewage farm in this country had been found after the experience of ten years to be profitable.

With regard to another question spoken of pretty confidently by the Author, viz., that irrigation was the proper mode of disposing of sewage, as yet nothing was satisfactorily settled, even respecting the sort of soil to be used, or how much sewage should be put upon the land—whether it should be that of one hundred persons, or fifty persons, or ten per acre per annum; and it was not known at what season of the year it was best to use it; nor was it ascertained what description of crop was best adapted for it. In point of fact the whole question was in such a state of uncertainty at the present moment, that no one was justified in expressing an opinion, in any such way as the Author had done, on the profitable or economical results of the application of sewage to land. Although the subject was well worthy the attention of engineers—it had received more than enough of the attention of chemists—it was to be regretted that it had not received such attention before Mr. Edwin Chadwick had taken it in hand, and had brought about the mischief which was now deplored, namely, the pollution of the majority of the rivers of England; for he was certain that the engineers would have found the means

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of disposing of the excreta of the population profitably to the land, economically to the population, and without injury to the rivers. In conclusion he would say that, according to his experience, if irrigation was to be adopted it must be after the sewage had been deprived of its sedimentary matter by some process of chemical precipitation, of which the phosphatic process appeared to be the most hopeful.

Mr. B. LATHAM observed that the strictures by Dr. Letheby were no doubt made in a conscientious spirit, but he could not subscribe to all that had been said. In fact, he had tried experiments with every process, chemical and mechanical, that had from time to time been placed before the public, with a view of effecting the purification of sewage, and he found that all of them failed to accomplish the object.

Coming to the question of the Paper, he had carefully perused the Report of the Rivers Pollution Commissioners, and he found this important omission—that no allowance was made in chemical experiments for the excess in volume of sewage after trying the processes. For example, in the A B C process a certain amount of water was mixed with the chemical compounds which were added to the sewage, which increased the volume of effluent water; the consequence was one volume of effluent water did not represent one volume of sewage. The amount of water after irrigation was considerably less in volume than the amount of sewage treated; consequently the tables in the Paper showed a return understating the purifying effects by irrigation, and overstating the purifying effects of all chemical processes.

The Author had dwelt very much upon the amount of purification effected by light and heavy soils; but if he had taken up any work on agriculture and studied it with the view to ascertain the amount of water which was capable of being absorbed by different soils, he would have found that clay soil would absorb three times the amount of a light soil; consequently the quantity of effluent water after the irrigation of clay soil was not so great as after the irrigation of sandy soil. So impressed was he with the errors that pervaded the Report of the Rivers Pollution Commissioners, which were now reproduced in the Paper, that a few days after the publication of that Report he wrote a letter to the Commissioners on the subject, and pointed out, from their own figures, the errors they had fallen into.

He would draw attention to the analyses of the sewage of various towns; and it would be seen from the Report, in all those cases in which the effluent water was not mixed with sub-

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soil water, that chlorine, which was absorbed in the least degree by land, appeared in greater quantity in the effluent water after sewage irrigation, than the sewage originally possessed. It was well known that in a small degree the soil absorbed it. How then could there be this excess of chlorine in the effluent water over and above what the sewage itself contained? The fact was the effluent water represented but a small proportion of the whole volume of the sewage.

The first Rivers Pollution Commissioners had clear views on this point; and Mr. J. T. Harrison, M. Inst. C.E., came to him at Croydon and suggested the propriety of gauging the quantity of sewage that went upon the farm, and also that which came off, in order to ascertain the degree of concentration of the impurities in the effluent water. He did so, and found that at all periods except on rainy days the volume of effluent water after irrigation was considerably less than the volume of sewage. In June last year the effluent water at Beddington represented from 1¼ to 1⅓rd the volume of sewage; but at Norwood the effluent water represented 4 volumes of sewage, showing that the degree of concentration of impurities in the effluent water at Norwood was at least 3 times greater than at Beddington. Therefore to draw the conclusion, from the analyses of the Rivers Pollution Commissioners, that light soil was better capable of purifying sewage than heavy soil, was opposed to actual fact.

With regard to the amount of evaporation of pure water on an irrigation area, he had made some crucial experiments, and had grown a plot of grass, carefully measuring every drop of water that went on and every drop flowing off, and he had found that for every pound of solid matter fixed on the grass, in round numbers 3,000 lbs. of water were evaporated. The evaporation from a square yard of growing grass varied from 2,880 lbs. in the months of June and July, 1870, to 30 lbs. in the months of January and February, 1871.

He might now say with regard to subsoil drainage, which was referred to in the Paper, that within the last few months he had made what he might call a discovery—for he found from actual experiment that there was a circulation of water in the soil, not entirely due to, but somewhat independent of, gravitation, for he found in an experimental tank that in the course of a day while by gravitation only water would penetrate 1¼ inch, it actually penetrated to a depth of 7 inches, pointing to a force at work, in addition to gravity, which might no doubt be due to capillary attraction.

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The great point insisted on by Dr. Letheby was that sewage irrigation lacked success, and that the deposit of sewage upon land for any length of time was a nuisance to those living in the neighbourhood. But he was certain it could be shown that the evidence led to conclusions in an opposite direction; and that there was no necessity for laying down a rule that sewage farms were not to be located to the south-west of a town. The Croydon farm had been under sewage treatment for upwards of eight years, and the local authorities had not lost by the undertaking. The farm was now let to a company. On the previous Friday that company had declared a dividend of 15 per cent. to the shareholders and carried forward another 15 per cent. to the next year's account, showing a profit of 30 per cent. on the year's outlay.

Mr. BAZALGETTE said Dr. Letheby had stated that sewage irrigation was neither profitable nor effectual. If Mr. Latham left the latter allegation untouched, it might be inferred that the river Wandle was polluted by the water discharged into it from the adjacent land.

Mr. B. LATHAM replied that the Croydon Local Board were proprietors of the Wandle for a length of about 800 yards on one side only of the river, and they had accepted an offer, at the rate of £120 per annum, for the trout fishing for that comparatively short length.

Dr. A. VOELCKER remarked that the practical question which presented itself to his mind, the question which pressed very heavily on many town authorities, was what could be done with the sewage? They had got it; whether it was valuable or worthless was another question: but what could be done with it? He was sometimes called upon to give advice, and he was often puzzled to know what was the best plan of getting rid of what he was inclined to think was now generally recognized by town authorities as a great nuisance. As long as men regarded the sewage as a mine of wealth he was inclined to think they would be far from the solution of the very difficult question how to dispose of it in the most profitable, or rather in the least expensive way. He would not say he was either an advocate for the dry plan, or an indiscreet advocate of the various plans for precipitation or purification by certain additions, which removed portions of the excremental matters in sewage; neither was he an indiscreet advocate for irrigation. All these plans according to circumstances might be advisable. In some cases he recommended irrigation; in others precipitation; in others he had recommended the discharge of the

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sewage by the most direct way into the sea as the most profitable for the town authorities.

In speaking of the chemical modes of treating sewage, he thought it should be clearly understood what had been accomplished by the various chemical processes which had been recommended for the purification of sewage. He ventured to say that no purely chemical process that had yet been suggested was at all capable of removing anything more than the suspended matter from the sewage. None of the valuable soluble fertilising matters could be removed in any quantity by any of the chemical processes with which he was acquainted; and it was well to bear in mind that the suspended matter formed but a very small portion of the real fertilising value in sewage. It must not, therefore, be expected by any chemical treatment of the sewage to obtain in a portable form the soluble matters which were the most valuable fertilising constituents of sewage. At the same time it was well to bear in mind that the suspended solid matters were the main cause of the nuisance which sewage was apt to create by irrigating the land, especially if the land was of a stiff and retentive character, which did not allow the excrementitious matter to become rapidly absorbed by the soil. He could therefore conceive it might be necessary in many instances to purify the sewage by preliminary precipitation; and it was satisfactory to know that by some of the processes, and more especially by the process recently introduced by Messrs. Forbes and Price, sewage could be deodorized and purified to a great extent. As his name had been mentioned as having given an estimate of the value of the solid manure that might be extracted by this mode of treatment, he would say that the value of the deposit depended very much upon the amount of valuable materials put into the sewage, and not upon the matters which could be obtained from sewage itself. If, for instance, £3 worth of phosphates were put into sewage a more valuable manure would be obtained than if only £1 worth of phosphate of alumina was employed in clarifying sewage. He should not like this remark to be regarded in disparagement of the process. Indeed he did not know any process which so rapidly and so thoroughly deodorized and clarified sewage as phosphate of alumina. The effect of a solution of phosphate of alumina upon foul sewage was very remarkable, and in his experience Messrs. Forbes and Price's plan was an efficient process of effecting the clarification of sewage without detracting from the subsequent value of the sewage for irrigating purposes. It was well that this should be put in a clear light, and that no exaggerated expectations of this process should

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be entertained; and it was for this reason he had mentioned that it could not be hoped to get by this process much fertilising matter of any value from the sewage itself. There was obtained by it a little nitrogenous matter from the sewage; but the chief value of the deposit depended on the quantity and the value of the materials put into it.

In some passages of the Paper it was stated that almost every description of land was equally well adapted for the reception of sewage. He rather demurred to that statement, because he thought it was most essential for the proper disposal of sewage, that the land should be of a porous character, so as to admit of the infiltration of air, which, after all, was the great purifier of those matters which rendered sewage obnoxious. Clay soils, as was well known, would absorb fertilising matters better than sand. What it was required to accomplish, however, was not merely the absorption of fertilising matters, but more particularly the rapid destruction of noxious matters; and this destruction could not take place in many clay soils as they were found. Many soils in the present state of cultivation were totally unfit for the reception of sewage, and until very material changes were made in the general agriculture of the country, until greater attention was paid to deep drainage and deep cultivation, he believed in many soils sewage irrigation was likely to create a nuisance. In looking out for land for the disposal of sewage, he would select light and porous soils as the most appropriate kind on which to put the sewage. Then again it was stated that probably the most profitable way of disposing of sewage was to put it in small quantities upon the land. As far as experience went the most economical results had been obtained by putting large quantities of sewage on the land; and he thought endeavours should be made to use as much sewage per acre as possible in practice. This was a matter of importance to town authorities who had to pay heavily for land which they took in hand. Generally, the less the acreage of land on which the sewage could reasonably be put, the more profitable would the application prove, both to the individual occupier of the sewage farm and to the town authorities.

Mr. E. Chadwick, C.B., remarked on the mechanical part of the question, that he had himself at different times examined the outfalls of various sewers of old urban districts, amongst others the outfall of the City of London; and he never saw from that or any of those outfalls either fæces or paper. What only was seen was a chocolate-coloured, fine powder mixture flowing out. How came it that there was nothing, not even paper, in form?

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The fact was that the matters were detained either in cesspools or in house drains of deposit, or in branch or in main sewers of deposit, until they were disintegrated by decomposition; and that decomposition, by the noxious gases evolved, and diffused by gulley shoots into the streets, and untrapped or ill-trapped drains in the houses, was at the expense of the health of the population. As a rule, a great part of the fine disintegrated matter was discharged when loosened by rainfall. The products of the putrid decomposition, thus wasted, were fine manurial products. By tubular, properly-adjusted, self-cleansing house drains, and properly-adjusted, self-cleansing tubular sewers, and a new sanitary engineering, another state of things was brought about. When the self-cleansing tubular system was laid down at Croydon, people for the first time saw fæces and paper coming out in abundance at the outfall, and many were disgusted at the large coprolites floating down the Wandle, where certainly they ought not to have gone. He, however, was delighted at the sight for he saw in it evidence of the fæcal matter coming out, comparatively fresh, unwasted by decomposition, and the population relieved from its products, as shown in that instance, by a greatly reduced death rate. Dr. Letheby should be informed of the difference—the enormous difference—between sewage which was putrid, like that from old districts such as the City of London, and sewage that was fresh, derived from self-cleansing new tubular drains and sewers. For, regarding the population of the rivers, which seemed most to concern many people, whilst old sewage that was putrid killed fish, sewage that was fresh fed them. At Carlisle, the people declared that under the new sewerage system, the fish were not only increased in quantity but were greatly improved in quality. Anglers now found their finest sport at the mouths of the new tubular systems, discharging the sewage fresh. This difference between sewage which was freshly discharged before advanced stages of decomposition could commence, and sewage which was putrid, was a large organic condition, governing the whole question of the internal as well as the external treatment of sewage. The work of the engineer might now be tested by the nose. If the smell of decomposing matter arose from house drain or sewer, there was stagnancy, decomposition, and bad work. In like manner the sanitary administration of a district might be tested by the nose, as also by the prevalence of foul air diseases amongst the population. If he were in office, and any case came before him of a district regularly pervaded by the smell of animal and vegetable decom-

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position, if it appeared that the medical officer had overlooked it, and had done nothing for its removal—if it appeared that he had favoured the retention of cesspools, or fæcal matter amidst houses, rather than discharge it at once into the river, and that foul air diseases prevailed, he would say to him unhesitatingly, "You are incompetent to the proper performance of your functions. You must go!"

With respect to the Paper, he considered it to be a very good summary of information on some of the leading points, but defective on the organic point of the external as well as the internal treatment to which he had referred. For, whilst the immediate and constant removal of refuse from beneath houses and the sites of towns was of primary importance for the prevention of the waste of manure as well as the waste of life, the immediate and constant application of the matter was of primary importance for good agricultural production. In this point of view all detention, all stagnancy, all surface decomposition, all foul smells on the field denoted waste and bad husbandry. The irrigation by the old water meadows, by the method of submersion, as at Edinburgh, at Milan, and the Clipstone meadows, were all objectionable on the score of the excessive expense of the works and the waste of manure, as well as on the score of health by the creation of evaporating marsh surfaces. The advances that had been made in agricultural economy and efficiency by the methods of distribution, by infiltration, at Romford, at Aldershot, and at Bedford, should be distinctly examined and displayed with the aid of the science of the horticulturist versed in the feeding of plants, which was always the best done in the liquid form. To save the loss as well as the waste from the decomposition of solid manure, commonly applied as top dressing, the best method was to liquefy it, and if the application were repeated in suitable quantities to the absorptive power of the soil, evaporation and nuisance, as well as waste, were immediately prevented. In twenty minutes after a proper dose of sewage manure had been applied to a prepared soil, there was no smell to indicate what had been done. Hypotheses had been promulgated about the propagation and distribution of parasites in liquid manure. What warranty was there in horticultural or other long practice, or in vegetable physiology, in liquefied culture, for such hypotheses? It was clumsy work to shed manure on leaves. Were those who put forward those hypotheses aware, on the contrary, of the extent to which vegetable and animal matter in its condition of solid manure served as nests for animalcules, and of the insect life that was favoured and propagated by it?

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Those who spread and exposed solid dung did not know the extent and sorts of larvæ they were distributing that would be killed in water. He had recently been asked, by an officer of the Colonial Department, to suggest some means of preventing a very serious inconvenience that had been experienced in the West Indies from the use of the dry earth closets; for the dry earth, with the dung, had been found there to be a very convenient habitat by some species of flying insects which bit ferociously, and which were found to be as dangerous as wasp nests to approach uncovered. The only remedy he could suggest was that of drowning them, by the adoption of the principle and the really cheaper process of the water-closet. By proper liquefied manure applications to land the entomology instead of being augmented was diminished, as shown by long experience, and as shown in advanced horticulture, the new conditions were unfavourable to the noxious parasitical growths apprehended. He had not time to enter into details as to some omissions in the Paper; as the omission of any notice of a plan he had directed of distribution by moveable carriers, by flexible tubes with lateral apertures which had succeeded in France; and he therefore confined himself to the assertion of the principle of the great organic distinction between sewage which was fresh and sewage which was putrid.

Mr. J. Newton, on the subject of the alleged ill-effects to health from sewage irrigation, stated that during the last three months he had applied the sewage from a town of ten thousand inhabitants on 10 acres of land, or at the rate of 36,000 tons per acre per annum; and that had been done without injury to health, with no smell, and with a result very beneficial to the land; but it was not his intention to continue the application of so large a quantity of sewage per acre. He stated the fact for the purpose of showing that no rule could be laid down determining the number of acres required for a given population; and more especially to prove that large quantities of sewage per acre did not necessarily create a nuisance. The land in question was situated on the south side of Carrington Moss, in Cheshire, and consisted of peat soil, from 12 inches to 18 inches thick, resting upon about 3 feet of coarse sand, with a clay or marl substratum. The sewage was applied to rye grass, on the ridge and furrow principle, the ridges being 40 feet apart, and the whole drained with 3-inch tile drains, laid along the furrows, at an average depth of 4 feet. With a clay surface it was certain that so large a quantity could not have been judiciously applied; and whilst the proportion of one hundred people per acre, as stated by the Author, might be taken as an

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average, he believed the extremes would vary from fifty to two hundred. In regard to the distribution of liquid sewage, he would strongly advocate the ridge and furrow system in preference to any other.

Mr. Bailey Denton believed that, by the proper preparation of land for sewage irrigation, all the objections raised to the practice might be avoided, and the differences of opinion now existing be reconciled. Up to this time more attention had been paid to getting rid of a nuisance with assumed profit, than to the appropriate treatment of the land to receive the sewage and benefit by it; and it was to the absence of underdraining, and to excessive 'flooding,' instead of watering, the soil, that might be attributed the evils and the objections which prevailed.

As the result of close observation he desired to record the following conclusions. First. All land, whether irrigated for profit or used for intermittent-downward filtration, must be either naturally absorbent, or must be rendered so by under-drainage, designed for the purpose, and appropriate surface treatment.

Secondly. No sewage whatever, when once applied to land, should flow off the surface into rivers and watercourses, and none should pass from one surface to another, by what was called the 'catch-water' system of application, unless the inclination should be so steep as to prevent its absorption.

Thirdly. With land thus naturally, or artificially, absorbent, any excess applied to it (beyond that which was necessary for saturation) would filter directly downwards, and escape by the underdrains, or pass to the subterranean water level, and there would then be no sewage detained on the surface. Hence no smell could arise from the land, nor could the dreaded parasites, of which so much had been said, cling to vegetation above the surface.

Fourthly. The whole of the sewage applied to land should pass through the soil in quantities, and for periods of time, regulated by the object aimed at. If the object were the profitable production of crops, then complete saturation of the land, at frequent intervals, was the desideratum, and repletion was waste. If, on the contrary, the object were the cleansing of the sewage, and its discharge in such a condition as to be admissible into rivers, then the quantity of land through which the sewage must filter, and the intervals between application might be greatly reduced, and might approximate to the quantities and times set forth in the recent Report of the Rivers Pollution Commission; though to avoid the chance of nuisance arising from a concentration of sewage, it would be desirable not only to divide the filtering area into beds,

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allowing of an interval of rest each day, but to have several series of filtering beds, allowing of a lengthened period of rest in the use of each series.

Fifthly. Land might be too free for profitable irrigation as well as for purification.

Sixthly. Clay lands efficiently underdrained and efficiently—*i.e.* deeply—cultivated on the surface, were quite as suitable as, and even more profitable when used for cropping than, gravelly and sandy soils. Undrained clays were altogether inadmissible.

And seventhly. That all sewage should be freed from its solid particles before it was applied to land either for irrigation or for filtration.

Mr. A. JACOB explained that Figs. 1 and 2, Plate 19, represented the analyses taken over one year, from the 24th of September, 1868, to the 24th of September, 1869, of the effluent water at Beddington and at Norwood. He had represented the results graphically to exhibit the amount of coincidence there was in the purification on clay lands as represented by Norwood, and on free and open soils as represented by Beddington. In Fig. 2 the light-shaded portion represented the whole of the organic carbon in the sewage before distribution over the land. Dr. Frankland, in the Report of the Rivers Pollution Commission, represented organic carbon as the carbon resulting from the decomposition of organic vegetable matter in the sewage; and the darker shading represented the total quantity in 100,000 parts of crude sewage. As the effluent water passed away the amount of organic carbon was represented by the darker bands, the lower one representing the amount at the Beddington outfall and the other at the Norwood outfall. Now in those two bands there was a certain amount of coincidence; and though Dr. Frankland had, to some extent, accounted for the variation in the amount of impurity, by stating that it might in some degree be influenced by frost, and especially by such a frost as occurred on the 28th January, and by the discharge of sewage through fissures in the ground, it would be perceived that the similarity in the results obtained at Beddington and at Norwood rendered such a conclusion doubtful. He could not concur in Dr. Frankland's mode of reasoning. He had plotted the rainfall for every day of the year; and it appeared from the diagram (Fig. 2) that the rainfall had no influence whatever on the amount of purification exercised by the soil or by the plants: for it would be noticed that on the 17th of December, after a heavy rainfall, there was a decrease in the amount of organic carbon, and it was the same with the nitrogen. Whether this was accounted for

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by the soil being more active, or by the effluent water being more diluted, was a matter of no great importance. It would suffice to say that the result was equally satisfactory whichever view was taken.

Mr. Hemans, Vice-President, said he trusted Mr. Hope would state the results of his large practical experience in this matter. The Paper had been met by a most impetuous attack by a high authority; and though he had a great respect for chemists, he had a higher respect for practical men who, like Mr. Hope, had devoted time and money to the elucidation of the question. For his own part he believed that great value was to be derived from the utilization of sewage, and he should be sorry that any sweeping condemnation of the whole system should pass by unnoticed.

Mr. W. Hope, V.C., said he had great pleasure in expressing his general approval of the Paper. Dr. Letheby had informed the meeting that he had no hesitation in stating that the Author had not visited the places described. He could speak positively with regard to one of them, as he had had the pleasure of a visit from Mr. Jacob to his own farm at Romford. He thought there was another statement which Dr. Letheby had made with as little degree of hesitation, viz., that, with one exception, no sewage farm ever had paid, or ever would pay, commercially. Now he paid the Local Board of Romford £600 a-year for the sewage which they manufactured, in addition to the rent of his farm; therefore, if Dr. Letheby was correct, he must sooner or later appear in the "Gazette" in a very unpleasant position; but he had no grounds for believing that so unfortunate an event was about to take place. There were very few points in the Paper on which he need offer any remarks. He agreed with the greater part of it: but with the remarks upon the phosphate of alumina process he did not agree, nor did he agree with the prospectus recently issued by the company that manufactured it. Of course it was the province of a prospectus to make out a good story, and that prospectus had certainly made a very good story. The directors quoted the analyses and report of Dr. Voelcker with regard to the worth of the deposit submitted to him, in which it was stated that its value was about £7 7*s.* per ton; but it was delivered to him in a dry state, containing only about 3 per cent. of moisture. Now the deposit from sewage, however produced, whether by a purely mechanical process or by a process which Mr. Hawksley described as a chemico-mechanical process, was always in the same condition of wet sludge, containing 80 or 85 per cent. of water, and before the product could be brought into the state in which Dr. Voelcker

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analysed it, a very expensive process must have been performed to drive out that water: therefore the value of the product, as he found it, had nothing to do with the profit to the Company who manufactured it; and it would be recollected that Dr. Voelcker stated that the bulk of that value had to be put in and was not taken out of the sewage. In the prospectus, which Mr. Jacob referred to, the promoters of the Company had, moreover, the honesty to confess that the addition of soluble phosphate could not render the effluent water less valuable, yet the mysterious addition of a small quantity of what they called milk of lime was to render it both valueless and innocuous. But there was no such process known to chemistry as the robbing of water of a microscopic quantity of ammonia held in attenuated solution. Numbers of chemists had tried it; but it had hitherto been found as impossible as squaring the circle, and the notion that the addition of a small quantity of milk of lime would effect so extensive a chemical revolution had no foundation on facts.

The Author had referred to an assertion once made, he did not know when or by whom, that 1 acre of land was sufficient to utilize the sewage of a hundred persons. That was a mere guess, and not an approximation to the truth; and in this, as in all similar matters, there was only one road to learning—that was, patience and study. It was impossible to lay down any abstract rule that 1 acre would utilize the sewage of so many persons per annum. It depended upon climate, soil, and crop: but he did not suppose any soil, climate, or crop could possibly utilize the entire sewage of a hundred persons continuously. The sewage of a hundred people contained nitrogen equal to what was yielded by 3 tons of the best Peruvian guano; and he was sure no farmer or chemist, would recommend 3 tons of Peruvian guano to be applied to 1 acre in the course of twelve months.

He believed that in proportion as sewage irrigation was popularised and extended, so the proportion of land to population must be extended. When there were only two or three farms in the neighbourhood of London a large quantity of sewage might be applied, because there were no competitors in the market, and the produce could be sold at fancy prices, as at Croydon; but the more sewage irrigation was extended the more general would be its application to other crops; and if so it would be unnecessary to apply this large quantity to 1 acre of land. It might be done at times: it might be done one year, but the following year none, or very little, must be put on the land. The conviction had been forced upon him, by observation and experience, that in winter

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sewage must be applied to fallow land or to land in which winter wheat, or oats, or beans had been sown. Scarcely any other crop could utilize the sewage. He thought, as a rule, farmers in country districts must in winter apply their sewage to cereal crops, to which, in the first instance, the chemists who conducted the original experiments found the sewage was not applicable. They conducted their experiments solely as chemists: they left out of sight all physiological questions, almost all agricultural questions, and still more practical farming questions, and they accepted as an axiom, that no crop could practically utilize sewage except grass. He believed that was a mistake. His own experience tended to show that Italian rye grass was one of those which gave the least return for the sewage used. Sewage had been treated in bulk, pumped up in bulk, and applied in bulk; those who first spoke of it talked of it as worth so much per ton; but the value per ton was a mere accidental question, dependent upon the local circumstances of each town. The only proper datum for calculation was the unit of population; that was, the source producing the sewage. Many medical and chemical men of the highest reputation and experience had made almost a special study of this point, viz., What was the value of the excreta of a mixed population during twelve months? The average of a vast number of such investigations indicated this to be 8*s.* or 8*s.* 6*d.* per head of population, men, women, and children together; and that was exclusive of the additions which must arise in the case of large towns like London and Liverpool, from the numerous horses kept for the conveyance of people and the delivery of goods. In fact, the larger the town the greater was the value of the sewage per head of population, though not necessarily per ton. There were circumstances under which there might be so much water in the subsoil, that though the water supply was small the number of tons of sewage per head of population in the year might be large. Here he would mention two useful formulæ: 30 gallons of water per diem per head of population was equal to 50 tons per head per annum, less 4 per cent.; and if the number of cubic feet per diem representing the total flow of the sewage of a town were ascertained, that, multiplied by 10, plus 2 per cent., gave the number of tons per annum.

Two systems of filtering had been alluded to, the one upwards and the other downwards. He distrusted both of them, at all events in the case of small towns, and in the bulk of instances engineers had to deal with small towns. Neither was cheap enough, because these things must be looked at from a money

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point of view. When he took a lease of the sewage of the town of Romford, the sewage had been diverted from the brook and conveyed to the farm near the town, where it was to be applied. The local authorities had erected some cheap engines obtained by public tender which worked badly and unsatisfactorily, and they had carried an underground pipe to what was considered the highest part of the farm; but there were so many banks and trees that it was nearly impossible at that time to ascertain the levels. He found it was not the highest part, and there was no plan of distributing the sewage. The authorities were unable, owing to one of those Acts which were sometimes unaccountably passed by the legislature, to grant him a longer lease than seven years; they were equally unable to make up their minds to spend money; but at last he succeeded in persuading them to spend a small sum, under his direction, for the erection of such works as were absolutely necessary for delivering the sewage to the highest part of the farm. The question of storage was one which also had to be taken into consideration. He introduced an obligation into the lease that they should pump the sewage produced in twenty-four hours in ten hours. They had no idea how this was to be done, nor had he; but they came under the obligation to do so, and that satisfied him. They were also to pump the Sunday's sewage on Monday. He then suggested the desirability of constructing tanks of sufficient capacity to hold this sewage, and to provide some apparatus to strain off the grit which wore out the fans of the centrifugal pump about every three months; the expenses for repairs were heavy, and amounted to about 100 per cent. He designed two tanks to be constructed of concrete with a wall between, and they were put up to public tender. His own estimate was £400; the highest tender was £2,736 and the lowest £700, and there was nothing left but for him to become a contractor, and to construct the tanks on his own estimate, which he did. He also constructed a system of sluices, which, instead of being made in one piece, opening from the bottom, thereby creating a great scour under the pressure of a head of 8 feet of water, were divided into a number of small pieces about 1 foot each deep; the bottom sluice was smaller. In front of the sluice so built up from the bottom on a narrow passage he dropped a shutter, which kept back all the floating matter; the shutter being dropped half above and half below the water. The result was that the sluice opening from the top kept back all the heavy solid deposited matter, and the shutter dropped down in front kept back all the floating matters; and when the last portion

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of the sluice was lifted, the shutter, having a grating fixed to the under edge, was dropped down to the ground, and by that means the whole of the solid matter was kept back. The tanks were filled and emptied alternately. The sluice was pumped by a chain pump, and the contents either conveyed away to another farm he had in the neighbourhood, or applied to such portions of land as appeared to require it. The bulk of the phosphates of sewage was contained in the fæces or solid matter; therefore, if the sewage was robbed of the suspended matter it must be replaced by artificial manure or it must be restored from time to time.

It had been said by Dr. Letheby that there was hardly any crop fit for sewage irrigation except Italian rye-grass; but, as he had already stated, he had found most other crops could be grown to greater profit; he might mention cabbages, cauliflowers, lettuces, and all green crops with luxuriant foliage. He had also grown successfully potatoes, which he did not expect to do; also onions and every kind of market-garden produce. In conclusion, he would say that he thought the Paper was a useful one, and not calculated to mislead.

Mr. R. Rawlinson, C.B., said he would begin by bearing testimony to the clearness and ability displayed in the Paper: also by expressing his regret that so violent an attack should have been made upon the Author at the commencement of the discussion. He remembered when certain works, in which the President and other members of the Institution had been largely engaged, were as much ridiculed and had as much doubt thrown upon them as that of town sewerage and sewage irrigation; he referred to the construction of railways. When, therefore, persons rose and condemned a thing because failures had been made, and from those failures came to the conclusion that it never would succeed, he considered they were bad prophets indeed. Such prophecies had been made with regard to railways; and if the full details of blunders in connection with railways could be faithfully written and truthfully embodied, it would make a volume of no small size. It was injudicious, therefore, to throw indiscriminate discredit upon town sewerage, house drainage, and sewage irrigation.

The tendency of modern civilization was to mass mankind together in towns. When so massed together they must have water for domestic uses; and if water was brought into a town it must be taken out again after it had been used; and there were only two methods by which that could be done, both of which were more or less carried into practice, viz., either by surface gutters (channels), as was notably the case on the Continent, in

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the capital of Prussia for instance, or by underground sewers and drains as largely carried out in England. It was only recently, however, that in this country sewers had been adapted to the purpose of receiving direct and carrying away fæcal matters, though the science of sewerage was older than the Roman empire. It was not more than thirty or forty years since English sewers had been adapted to their full purpose; and he was not sure whether the prohibitory clauses in private Acts had been repealed, by which the passing of effete matter from water-closets was forbidden. It had been certainly forbidden in the metropolitan Act, and he knew it had also been the same in the Liverpool Improvement Act, as also in the private Acts of other towns; and it was only recently towns had adopted the practice of passing effete matter by drains direct from houses into sewers. Dr. Letheby had reflected on the former Board of Health because, as he said, they had poisoned rivers by not making due provision for the discharged sewage; they closed cesspools and thereby polluted rivers. He, however, differed altogether from such conclusions. London was in the beginning of this century a cesspool city; the first sewers made were not used to convey sewage, but only surface water. The mortality of London was then very high; and when the sewers were made to receive and convey away the putrid overflowings of the cesspools, and water-closets were drained into them direct, they were not properly ventilated, and typhus fever broke out in the sewered streets. Since then cesspools had been abolished in London—not by hundreds but by thousands and tens of thousands; and the sewers had been fully ventilated and made to do the proper work of sewers. The river Thames, it was true, was polluted to an enormous extent; but the lives of thousands of people had been saved. Therefore he urged the necessity of getting rid of putrid sewage, even if it were at the expense of the purity of rivers, if human life was the prime consideration in the problem to be studied.

Then he came to sewage irrigation; and here again this question had to elbow its way into public notice. It was met by persons who ought to assist with attempts to thwart it. Sewage irrigation was denounced as making sound land swamps, and so breeding pestilence; as being enormously expensive without any corresponding benefit, and all manner of evils, it was said, would come from applying sewage to land. Look, however, at the facts. It was true there were not many successful sewage farms as yet in the country. It was also true a great many mistakes had been made; and it was further quite true that mischief might have

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accrued from an improper use of sewage in some cases; but all these things were matters which engineers and scientific men must correct. It was not necessary that land should be made a swamp by putting sewage upon it; it was not necessary even to create a nuisance by putting sewage upon the surface of any land; nor was it necessary, in all cases, to work a sewage-farm at a loss. A sewage-farm might be carried on without causing nuisance, and yet with a profit at the end. Mr. Hope had described what he had done, and those who wished to see good sewage farming should go to Romford. As to the quality of land, there was no class of land, from the blown sand of the desert to the heaviest clay—there was indeed nothing except rock—that would not be improved by sewage cultivation, that could not more or less be benefited by the application of sewage; and there was no crop grown for the use of men or cattle, as market garden produce or Italian rye grass, that would not be benefited by the application of sewage. The bugbear, that it tended to the increase of entozoic diseases in the human frame, was absurd. The coarsest specimen of sewage farming was that which was practised at Edinburgh. There the putrid sewage came down without stint; there was no regulation whatever as to the volume put upon the land; the area irrigated was very limited compared with the volume of sewage used; the weight of grass grown on the irrigated land varied from 50 to 70 tons per Scotch acre per annum, which was about one-fifth more than a statute acre. Cows had been stall-fed on that grass for thirty years and more, and medical men had made careful examination in the hospitals of Edinburgh for the last sixteen years to see if they could detect any special increase in the hydatid taint, and they had failed to find any excess.

The money value of sewage was a moot point. Chemists said that the laboratory value was 2*d.* per ton; but the farmer, on the other hand, said that the year through he could not afford to pay more than ½*d.* per ton. Mr. Hope had observed that the great chemists and experimenters had only attempted to find the laboratory value of sewage as chemists; but he thought that was confuted by the fact that there was a Royal Commission, of which he had been a member, composed of chemists, medical men, engineers, and agriculturists, a member of the House of Lords, and of the House of Commons, and both the latter chosen because they were known to be farmers; and that experiments were carried out at Rugby by Mr. Lawes, with the assistance of Dr. Gilbert, who investigated the question in every phase—engineering, chemical, and agricultural; and the results were embodied in three

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blue books containing numerous tables. The sum total arrived at was, that the ingredients of town sewage, such as of the metropolis, were of the value of 2*d.* per ton taken at the most favourable period of the year; but, inasmuch as it had to be utilized every day, it could not be of greater value than ½*d.* per ton the year round. That price applied to the sewage of the Metropolis, would show an enormous annual value which was being poured into the Thames. Sewage-farms must be worked and cultivated to produce excessive crops as compared with ordinary farming, or they would be a failure; and he had no hesitation in saying that the increase in rent value ought not to be less than fivefold, that was 1 acre was multiplied into 10 in point of produce. There was, therefore, a great incentive to utilize sewage; and in doing that a larger amount of human labour must be employed, of which there was at present a redundancy. He held that there was high encouragement for perseverance in the utilization of sewage, and that there was no need to despair about fear of injurious pollutions, or of producing nuisances on the land.

Mr. F. W. SHEILDS said that the people most interested in sewage irrigation appeared to be divided into two hostile classes. He had heard eminent men give evidence that sewage irrigation, so far from being injurious to health, was the reverse, and that land in the neighbourhood of sewage farms would be readily taken up by persons anxious to build; while other men of equal eminence predicted all sorts of diseases to man and beast from the exhalations from the irrigated land.

He thought the truth lay between the two extremes. No doubt there was a strong prejudice, in any neighbourhood where sewage farming was proposed, against the establishment of sewage irrigation in that locality; and that alone was a good reason why precautions should be taken to divest the sewage, before applying it to land, of every element likely to create not only unhealthiness but nuisance. He had come to the conclusion that before long the legislature would pass an Act forbidding the application of sewage to land, until that sewage had been properly disinfected, and divested of all those elements which created nuisance by exhalation. Those elements he believed were much less active in the fluid sewage, especially when fresh, than in the deposit which the sewage left upon the land in the form of a thin, slimy coating. This under the action of the atmosphere was decomposed, and created far more exhalation than the sewage itself.

The question then arose, by what process could the sewage be best divested of those impurities which were contained in its

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solid matter? It was well known that the solid matter of sewage contained about one-eighth, and the fluid matter seven-eighths, of the manurial properties; while the proportionate quantities of solid matter to fluid were only one in two thousand parts by weight. The fluid, therefore, contained originally by far the larger portion of the fertilizing ingredients, including nearly all the ammonia, which no known chemical process could separate from it. When, therefore, the sewage was divested of the solid matter which it held in suspension, it was not only freed from the ingredients which tended to create nuisance and unhealthiness, but it was also rendered more suitable for irrigation by being cleared of the slimy matter, which, covering the surface of the ground, prevented the sewage from penetrating to the roots of the grasses or other crops, and it thus became practically more valuable for irrigation purposes than it had been in its original state.

The question remained, therefore, as to what process was most effectual in clarifying sewage and precipitating the solid matters within moderate limits of expense? The best process he had seen till lately was that which was practised, though on an imperfect scale, at Ealing, partly under the advice of Professor Way. The sewage was mixed with a proportion of lime-water, and delivered into settling tanks, through which it took about an hour and a half to pass. It then received a small dose of a mixture of salt of iron, and went through a coarse process of filtration, and the water flowed away in a state of remarkable purity considering the method adopted. By carrying out that process more carefully, which in fact would be equivalent to what the Rivers Pollution Commissioners had recommended, he had no doubt a considerable amount of purification might be effected, not only without depriving the sewage water of its fertilizing properties, but rather, on the contrary, by augmenting them. The sewage water might thus be rendered sufficiently clear either for discharging into rivers, or for application to the irrigation of land, without giving rise to any nuisance, even in residential neighbourhoods.

Another process, and no doubt a remarkable one, was that introduced to the public within the last few days by Dr. Forbes, and experimented upon on a large scale at Tottenham, viz., the precipitation of the solid sewage by the addition of a natural phosphate of alumina, previously treated with sulphuric acid, and of lime. As far as he could judge, the purification and clarification of the sewage water, and the deposit of all the solid matter in it, were effected very completely; and the effluent sewage water

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was clear and free from deposit. The value of the manure was stated by Dr. Voelcker to be sufficient to cover the expense of the process. If that were so it was in his opinion by far the most valuable chemical arrangement for the purification of sewage yet brought before the public. He thought, however, that it might be advisable to have the test of actual experience as to the value of the manure, as he believed that phosphate of alumina was not soluble by land; and any imperfect separation of the phosphoric acid from the alumina which might be found to occur, would proportionately diminish the value of the manure. He estimated that if the manure would fetch about £4 per ton, it would cover the expense of the process as he saw it performed at Tottenham, and leave the effluent water in a fit condition for passing into a river, in case land was not available at a moderate cost for irrigation; or if land were so available, it would leave the effluent water in a more valuable state than it was originally, and free from any tendency to create fever or nuisance.

Mr. C. E. AUSTIN submitted that sewage irrigation had been proved to be both economical and profitable. It had been stated that irrigation should not be carried out in situations to the south-west of a town. He believed the irrigation farm at Croydon was to the south-west of that place; but it could be proved by that farm that irrigation, properly carried out, was not detrimental to health; that ozone was produced by the proper management of irrigation,[1] and that the air when it came to Croydon over those fields was more healthy, in that it contained more ozone, than when it blew from any other quarter. The great point in performing the operation in a healthy and profitable manner seemed to be to bring the sewage to the ground before the solids were dissolved or decomposed. This could be done only by having good sewers, properly constructed, in which the solid matters did not lodge; and it was extraordinary that in some of the most modern towns, in the neighbourhood of London, the sewers were so constructed that the solid matter never appeared at the outfall, but was left to decompose beneath the streets in enormous unequally-laid receptacles called sewers, but for which tanks would be a much more appropriate designation.

The numerous propositions which had been propounded for collecting the sewage in tanks at the outfall, and then divesting it of its solids, were inefficacious in consequence of this detention of the

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[1] *Vide* "Physiological and Medical aspect of Sewage Irrigation," by Dr. Alfred Carpenter.

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solids in the sewers, or in the tanks, until they were dissolved; no chemical or mechanical process having as yet been invented which could precipitate or in any way separate the large quantity of matter then held in solution. It was said that the separation of the solid from the fluid was not necessary, and that the clarification of the fluid was of no importance. But it had been shown during this discussion that in order to produce the largest profitable result the solids and fluids should be utilized separately, that there was sufficient fertilizing matter in the fluid alone to serve the purposes of irrigation, and that the solids in a fresh state might be more advantageously employed separately for another description of crops. To effect this object, as well as that for which all sewers should be designed, namely, the disposal of town sulliage before decomposition, it was imperative that the sewers should be so constructed as to convey the sewage with the least practicable amount of friction, that it might arrive at the outfall as nearly as possible in its normal state. He believed Mr. Baldwin Latham had constructed a wheel which separated the solid from the fluid; and he should be glad to hear how that plan had acted at Croydon, and what profits had been made by it.

Mr. HEMANS, Vice-President, remarked that his experience induced him to lay no stress on the separation of solid from fluid sewage. If that principle were applied to the sewage of London there would be 200 tons or 300 tons per day to dispose of. He did not believe that any mercantile profit could be derived from such an enormous encumbrance as that, nor could he understand where this large deposit could be put; though it might be dealt with in a small place like Ealing. Mr. Hope had remarked that the process of separating the solid from the liquid would deprive the sewage of a valuable portion of its constituents; therefore it did not appear that it was desirable to go to the expense of such a process in dealing with the sewage of a large city. He believed the attempt would end in disappointment and commercial failure.

Mr. W. C. SILLAR observed that he looked upon the subject not only as one of depositing sewage and getting rid of a great nuisance, but also as a question of political economy. As the earth fed mankind, so in her turn must the earth be fed by man, that she might repeat the process of feeding her children.

To be assured of the fertility of sewage it was only necessary to visit a sewage farm. There was a good example at Tunbridge Wells. At the same time, owing to the limited area of the land

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under sewage cultivation, there was no doubt a great waste of material; and it was the duty of every one to bring what experience he could to bear upon the subject, to see whether some more economical means could not be adopted, by which this great waste of the whole sewage of a town being deposited upon a few acres of land could be avoided. He had personally superintended the works of the A B C company at Leamington; and he thought the accounts that had been given omitted a great deal of what was interesting. It was found that by mixing with the sewage a quantity of clay, animal charcoal and blood, precipitation took place, and that the precipitate, when dried, was rendered of a portable form, and capable of being sent all over the country, even to the very fields that originally gave the substance, thus literally replenishing the earth; and he could state that it contained fertilizing properties far in excess of what it had been given credit for. To prove its portability he had brought a box of it with him. That it contained fertilizing elements any one could test for himself; for which purpose he had also brought with him a small portion of caustic potash. It had been asserted that the deposit thus obtained was comparatively valueless; this was an error, for the deposit when mixed with quick-lime or caustic potash discharged an amount of ammonia which was palpable to the senses; therefore, without saying a word against irrigation, he contended that it was a great advantage to be able to take out the putrid elements from the water and to make a manure that was profitable and easy to furnish to farmers. The practical test of its value was that the demand for this manure was greater than the company could supply. The accounts which the farmers, who used it, gave were marvellous; and if it was objected that the whole of the fertilizing matters were not removed by the process, what was left in the water might be used to fertilize irrigation farms; the untreated sewage put upon those farms being generally more than they could advantageously absorb.

Mr. A. Jacob, in reply upon the discussion, said the propriety of assigning any given proportion of population to an acre of land had been questioned with some justice. It would be noticed that he had recognised the difficulty, by saying that a particular proportion would be sufficient to purify the sewage; but he did not say that the best agricultural results could be obtained from such a proportion. Mr. Hope advocated a smaller proportion, he believed about fifty persons to the acre, in order to get good commercial results; but he knew from experience that the sewage of a hundred, or even of two hundred people, could be utilized.

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He did not wish to allude to the remarks of Dr. Letheby farther than to the statement that he had drawn largely upon the Report of the Rivers Pollution Commissioners. He had designedly adopted the results of that Commission, because the gentlemen composing it were invested with authority by Government to investigate the subject impartially, and to communicate the results arived at from all the evidence they could collect; and when those results were given to the profession for their guidance they were bound to accept them.

The Ealing process had been alluded to, and Mr. Sheilds had stated what was correct in one sense, viz., that the discharged sewage water was apparently pure; but the application of a small quantity of lime, which was part of the process, suspended all putrefaction. It was well known that alkali of any kind introduced into sewage would suspend putrefaction for some time; but when once the sewage was discharged into a body of water, the alkali was gradually neutralized and then putrefaction rapidly set in. He thought the same objection applied to Dr. Forbes' process. Indeed, Dr. Forbes went so far as to admit, that it only extracted a small portion of ammonia, and that he considered the process as supplemental to irrigation rather than as a substitute for it. In some instances probably it might be made a valuable adjunct to irrigation works.

With regard to the separation of solid from liquid sewage, he strongly advocated a considerable degree of purification, because, though it was not admitted that the public health was likely to suffer from the effluvia from an irrigation farm, yet the senses of the public must be respected, and they must not be subjected to personal annoyance any more than to unwholesome influence. On that ground, therefore, he advocated careful filtration of the sewage; and Dr. Voelcker went the length of stating that absolutely filtered sewage was more valuable as a manure than sewage in the ordinary state or only partially filtered.

Mr. Vignoles, President, said he could concur in the remark that a great number of mistakes had been committed in railway-making and working; probably few persons were in a better position to bear testimony to that than he was. It was not merely in railways, however, or in other branches of engineering, but in every department of science; and practical men had always profited by the mistakes of themselves and others. He recollected one of the most eminent engineers of the age saying to him, many years ago, "I shall not care much about you till you have had two or three bridges or other works tumble about your ears."

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Bridges might be made too strong on the one hand, and yet, on the other hand, useful information was to be gained by any weakness displayed. Long as the question of sewage irrigation had been already discussed, it was far from being exhausted, and there was still a great deal to be learnt about it. He thought much of the success attained was due to the quality of the water impregnated with the various fertilizing matters. Water of itself was a great fertilizer; but the subject was a vast one, and he did not feel competent to deal with it; still he hoped the Paper and the discussion on it would be a source of future advantage to engineers.

May 23, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

THE following Candidates were balloted for and duly elected:—SANDFORD FLEMING, as a Member; EDWARD BANFIELD, PETER WILLIAM BARLOW, Jun., WALTER BRANDRETH BROMLEY, CHARLES TOLER BURKE, Stud. Inst. C.E., JABEZ CHURCH, Jun., Stud. Inst. C.E., CHARLES JOHN GENESTE, JOHN LILLYWHITE, JOSEPH NEWTON, and GEORGE SWETENHAM, Capt. R.E., as Associates.

It was announced that the Council, acting under the provisions of Sect. III., Cl. VII., of the Bye-Laws, had transferred ROBERT HARVEY BURNETT, JOHN CARRUTHERS, LEWIS WILLIAM PRITCHARD, and CHARLES HENRY WARING, from the class of Associate to that of Member.

Also, that the following Candidates, having been duly recommended, had been admitted by the Council, under the provisions of Sect. IV. of the Bye-Laws, as Students of the Institution:—THOMAS MILNES FAVELL, JOSHUA PERCY JOSEPHSON, WILLIAM MACDONALD MATTHEWS, JOHN NARCISO DE OLANO, and WILLIAM CORT STARIE.

The discussion upon the Paper No. 1,304—"The Treatment of Town Sewage," by Mr. Jacob, was continued throughout the evening.

In pursuance of the notice on the card of the Meetings, it was proposed, and resolved unanimously—

"That, in order to insure a fuller attendance of Members than could be obtained on Whit Tuesday, the Meeting be adjourned until Tuesday evening, the 6th of June."

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June 6, 1871.

The Session was concluded by a CONVERSAZIONE, which was given by the President to the Members of the Institution and to a numerous circle of distinguished visitors, when many choice Works of Art, and interesting Engineering Models and Instruments, were exhibited, of which the following is a Catalogue:—

CATALOGUE OF WORKS OF ART.

NOTE.—*This is a first attempt to bring together a collection of Paintings and Water Colour Drawings referring to the object for which the Institution was incorporated—"The advancement of Mechanical Science, particularly that branch of it called Civil Engineering, which relates, among other things, to the construction of Roads, Bridges, Railways, Aqueducts, Canals, River Navigation, and Docks; to Embankments against encroachments from the sea and from river floods; to the construction of Ports, Harbours, Moles, Breakwaters, Lighthouses, and Machinery; to Navigation by Artificial Power; and to the Water Supply and the Drainage of Cities and Towns."*

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1	ANCIENT MODE OF FORGING GUNS.	SIR J. WHITWORTH, Bart., M. Inst. C.E.
2	FROM UNDER THE SEA. *J. C. Hook, R.A.* .	C. P. MATTHEWS.
3	WATT, AS A BOY, OBSERVING THE POWER OF STEAM. *Marcus Stone*	W. H. WILLS.
4	PORTRAIT OF ARKWRIGHT, THE INVENTOR OF THE THROSTLE	SIR THOS. BAZLEY, Bart., M.P.
5	INVENTION OF THE STOCKING LOOM. *A. Elmore, R.A.*	SIR THOS. BAZLEY, Bart., M.P.
6	PORTRAIT OF CROMPTON, INVENTOR OF THE SPINNING MULE	SIR THOS. BAZLEY Bart., M.P.
7	THE TWO GRINDERS. *G. Sykes* . . .	JAMES GAMBLE.
8	VIEW OF DORT. *Albert Cuyp*	MISS BREDEL.
9	THE HARBOUR OF ST. IVES, CORNWALL. *E. W. Cooke, R.A.*	ALDERMAN SIR D. SALOMONS, Bart., M.P., Assoc. Inst. C.E.
10	WINTER LANDSCAPE. *Ruysdael* . . .	WYNN ELLIS.
11	BRIDGE OVER THE ELBE, AT DRESDEN. *Bernardo Canaletto.*	J. WHITEHEAD.
12	THE RIALTO, VENICE. *Canaletto* . .	THOS. BARING, M.P.

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14 THE BELL ROCK LIGHTHOUSE. *J. M. W. Turner, R.A.* (1819) — D. STEVENSON, M. Inst. C.E.

15 ENGRAVING OF BUST OF THE LATE ROBERT STEVENSON, M. Inst. C.E. . . . . D. STEVENSON, M. Inst. C.E.

16 THE IRON FORGE. *Wright, of Derby.* — THE RT. HON. W. F. COWPER-TEMPLE, M.P.

17 BRIDGE OF RIMINI. *R. Wilson* . . . WYNN ELLIS.

18 WIND MILL. *J. Ruysdael* . . . THOS. BARING, M.P.

19 THE PONT DU GARDE, NISMES. *F. R. Lee, R.A.* F. R. LEE, R.A.

20 SOUTHWARK BRIDGE. *Nash* T. BRASSEY, M.P., Assoc. Inst. C.E.

21 WESTMINSTER BRIDGE. *S. Scott.* — G. P. BIDDER, Past President Inst. C.E.

22 THE ST. GOTHARD PASS, WITH DEVIL'S BRIDGE. *J. M. W. Turner, R.A.* . . HENRY CARR, M. Inst. C.E.

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28 BIRD'S EYE VIEW OF A DUTCH CHÂTEAU. *I. Van der Heyden. The figures by Eglan Van der Neer.* J. C. ROBINSON.

29 AQUEDUCT OVER THE RIVER LUNE, NEAR LANCASTER. *Daniel* . . . . . G. B. RENNIE, M. Inst. C.E.

30 THE GRAND CANAL, VENICE. *Canaletto* . . WYNN ELLIS.

31 ENTRANCE TO THE ITALIAN ARSENAL OF SPEZZIA. *S. Clegg, Jun.*, M. Inst. C.E. — J. D'A. SAMUDA, M.P., M. Inst. C.E.

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54 SIR S. BROWN'S SUSPENSION BRIDGE OVER THE TWEED.
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54*PHOTOGRAPHIC VIEWS OF THE WORKS OF KIEFF SUSPENSION BRIDGE . C. B. VIGNOLES, F.R.S., President Inst. C.E.

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175	Chassepot Rifle	Colonel Dixon, R.A.
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178	Section of a Martini-Henry Rifle	War Office.
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195 Specimens of Wrought and Cast Iron Street Tramway, as laid in Buenos Ayres. J. LIVESEY, Assoc. Inst. C.E.

196 Wrought-iron Sleepers for Light Railways. J. LIVESEY, Assoc. Inst. C.E.

197 Winby's Reversible Rail and Chairs . . W. MAY.

198 Iron Elastic Permanent Way for Narrow Gauge Lines . . . . . R. MALLET, M. Inst. C.E.

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SPECIAL GENERAL MEETING OF MEMBERS ONLY.

June 27, 1871.

CHARLES B. VIGNOLES, F.R.S., President, in the Chair.

The SECRETARY read the notice convening the meeting, as follows:—

"THE INSTITUTION OF CIVIL ENGINEERS.
"*Established* 1818. *Incorporated by Royal Charter* 1828.
"*25, Great George Street, Westminster, S.W.*
"*15th June*, 1871.

"IN accordance with the request, of which a copy is annexed, the Council hereby convene a GENERAL MEETING OF MEMBERS only, for TUESDAY, the 27th inst., at Four o'clock precisely, for the purpose of considering and deciding upon the proposed alterations in and additions to the Bye-laws and Regulations.

"By Order,
"JAMES FORREST,
"*Secretary.*"

"WE, THE UNDERSIGNED MEMBERS OF THE INSTITUTION OF CIVIL ENGINEERS, in pursuance of the 1st Clause in the 14th Section of the Bye-laws and Regulations of the Institution, hereby request the Council of the Institution to summon a General Meeting of Members, to consider the expediency of making the following alterations in, and additions to, the existing Bye-laws and Regulations of the Institution, and, if approved, to adopt the same, that is to say:

"*Instead of* the words in Section II., Rule 6: 'The Officers of the Institution shall be a President, four Vice-Presidents, and *twelve* other Members of Council, being *seventeen* persons.'

"*Insert* the words: The Officers of the Institution shall be a President, four Vice-Presidents, and *fifteen* other Members of Council, being *twenty* persons.

"*Instead of* the words in Section II., Rule 7: 'The President, Vice-Presidents, and *ten* of the other Members of Council, shall be from the class of Members; the other *two* Members of Council shall be from the class of Associates.'

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"*Insert* the words: The President, Vice-Presidents, and *twelve* of the other Members of Council, shall be from the class of Members; the other *three* Members of Council shall be from the class of Associates.

"*Section* II., *following Rule* 7, *insert the following Bye-law*:—

Each and every year, two of those Members of the Council who are also Members of the Institution shall retire from the Council, and shall become ineligible for re-election as Members of Council until after the expiration of one year; and no Member of the Institution shall serve as a Member of Council, except as President or Vice-President, for more than six years consecutively, from or after the date of the 'Annual Election' in the month of December, 1871; and each and every year, two of those Members of the Council who are also Associates of the Institution shall retire from the Council, and shall become ineligible for re-election until after the expiration of three years; and no Associate of the Institution shall serve as a Member of Council more than two years consecutively.

The Council may determine, when necessary, which of its Members or Associates shall retire and become ineligible for re-election.

"*Instead of* the words in Section V., Rule 1: 'The list shall contain the names of at least *seventeen* Members, and at least *four* Associates, and shall be the balloting list for the Annual Election on the Tuesday next previous to Christmas Eve.'

"*Insert* the words: The list shall contain the names of *nineteen* Members (*of whom at least two shall not previously have served on the Council*), and *six* Associates, and shall be the balloting list for the Annual Election on the Tuesday next previous to Christmas Eve.

"George Berkley.
George B. Bruce.
J. D. Baldry.
Edwin Clark.
Jno. Coode.
E. A. Cowper.
Wm. R. Galbraith.
John Thornhill Harrison.
Harrison Hayter.
R. Jacomb Hood.
W. B. Lewis.
J. A. Longridge.
James Samuel.
C. W. Siemens.
J. H. Tolmé.
R. J. Ward.
R. Price Williams.
Frank C. Stileman."

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Mr. Cockburn Curtis Godsman protested against the legality of the meeting, on the ground that the additional rule proposed to be inserted after Rule 7, Sect. II., was repugnant to the Charter, inasmuch as its effect would be to limit and restrict the choice of Members to serve on the Council.

Mr. Coode proposed, and Mr. Cowper seconded, the following Resolution:—

"*Instead of* the words in Section II., Rule 6: 'The Officers of the Institution shall be a President, four Vice-Presidents, and *twelve* other Members of Council, being *seventeen* persons.'

"*Insert* the words: The Officers of the Institution shall be a President, four Vice-Presidents, and *fifteen* other Members of Council, being *twenty* persons.

"*Instead of* the words in Section II., Rule 7: 'The President, Vice-Presidents, and *ten* of the other Members of Council, shall be from the class of Members; the other *two* Members of Council shall be from the class of Associates.'

"*Insert* the words: The President, Vice-Presidents, and *twelve* of the other Members of Council, shall be from the class of Members; the other *three* Members of Council shall be from the class of Associates."

After discussion, the President called for a show of hands upon the Resolution, and declared the same to be carried.

Mr. Siemens proposed, and Mr. Berkley seconded, the following Resolution:—

"*Section* II., *following Rule* 7, *insert the following Bye-law*:—

Each and every year, two of those Members of the Council who are also Members of the Institution shall retire from the Council, and shall become ineligible for re-election as Members of Council until after the expiration of one year; and no Member of the Institution shall serve as a Member of Council, except as President or Vice-President, for more than six years consecutively, from or after the date of the 'Annual Election' in the month of December, 1871; and each and every year, two of those Members of the Council who are also Associates of the Institution shall retire from the Council, and shall become ineligible for re-election until after the expiration of three years;

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and no Associate of the Institution shall serve as a Member of Council more than two years consecutively.

The Council may determine, when necessary, which of its Members or Associates shall retire and become ineligible for re-election."

Whereupon it was moved by Mr. HARRISON, V.P., and seconded by Mr. BIDDER, Past President:

"That the proposed additional Bye-law be rejected."

After considerable discussion, the adjournment of the meeting was moved by Mr. J. W. BARRY, seconded by Mr. JOHN GRANT; and, upon being put from the chair, it was declared to be carried.

ADJOURNED GENERAL MEETING OF MEMBERS ONLY,

AND

SPECIAL GENERAL MEETING OF MEMBERS ONLY.

August 1, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

The SECRETARY read the notice convening the meetings, as follows:—

"THE INSTITUTION OF CIVIL ENGINEERS,
"*Established* 1818. *Incorporated by Royal Charter*, 1828.
"25, *Great George Street, Westminster, S.W.*,
"20*th July*, 1871.

"DEAR SIR,

"I AM instructed to inform you that the GENERAL MEETING OF MEMBERS only, which was adjourned on TUESDAY, the 27th ulto., will be continued on TUESDAY, the 1st of August next, at 12 o'clock, Noon, when, with the permission of the Meeting, leave will be asked to withdraw the suggested new Bye-law, proposed to be inserted after Rule 7, Section II., as well as the suggested alterations in Rule 1, Section V.

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"I am further to say that, when the business of the Adjourned Meeting has been concluded, your attendance is requested at a GENERAL MEETING OF MEMBERS only, for the purpose of considering and deciding upon certain alterations in the Bye-laws and Regulations, which are recommended by the Council, and which are specified at the end of this notice.

"I am,

"Yours faithfully,

"JAMES FORREST,

"*Secretary.*

"*Instead of* the words in Section V., Rule 1: 'The list shall contain the names of at least *seventeen* Members and at least *four* Associates, and shall be the balloting list for the Annual Election on the Tuesday next previous to Christmas Eve.'

"*Insert* the words: 'The list shall contain the names of at least *twenty-three* Members and at least *six* Associates, and shall be the balloting list for the Annual Election on the Tuesday next previous to Christmas Eve. *The names of persons proposed for Members of Council—other than for the President and the four Vice-Presidents—shall all be printed in the same character of type, and in alphabetical order, and the names of persons proposed for Associates of Council shall also be printed in a similar way.*'

"*Instead of* the words in Section IX., Rule 1: 'The Council shall consist of the President, the four Vice-Presidents, and *twelve* other Members of Council, who shall be elected according to the provisions of Section V.'

"*Insert* the words: 'The Council shall consist of the President, the four Vice-Presidents, and *fifteen* other Members of Council, who shall be elected according to the provisions of Section V.'"

Mr. BERKLEY (in the absence of Mr. SIEMENS, who proposed the Resolution at the previous meeting) moved, with the permission of the meeting, and Mr. PRICE WILLIAMS seconded, that the proposed addition to the Bye-Laws, following Rule 7, Sect. II., as well as the suggested alteration in Rule 1, Sect. V., be now withdrawn.

This having been agreed to, the President declared that the business of the adjourned meeting was closed.

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The Members present then resolved themselves into a SPECIAL GENERAL MEETING for the purpose stated in the notice, whereupon

Mr. CUBITT, Vice-President, proposed, and Mr. WOODS, Member of Council, seconded, the following Resolution:—

"*Instead of* the words in Section V., Rule 1: 'The list shall contain the names of at least *seventeen* Members and at least *four* Associates, and shall be the balloting list for the Annual Election on the Tuesday next previous to Christmas Eve.'

"*Insert* the words: 'The list shall contain the names of at least *twenty-three* Members and at least *six* Associates, and shall be the balloting list for the Annual Election on the Tuesday next previous to Christmas Eve. *The names of persons proposed for Members of Council—other than for the President and the four Vice-Presidents—shall all be printed in the same character of type, and in alphabetical order, and the names of persons proposed for Associates of Council shall also be printed in a similar way.*'

"*Instead of* the words in Section IX., Rule 1: 'The Council shall consist of the President, the four Vice-Presidents, and *twelve* other Members of Council, who shall be elected according to the provisions of Section V.'

"*Insert* the words: 'The Council shall consist of the President, the four Vice-Presidents, and *fifteen* other Members of Council, who shall be elected according to the provisions of Section V.' "

After discussion, the motion was put from the Chair, and declared to be passed unanimously.

A vote of thanks to the President was carried by acclamation.

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